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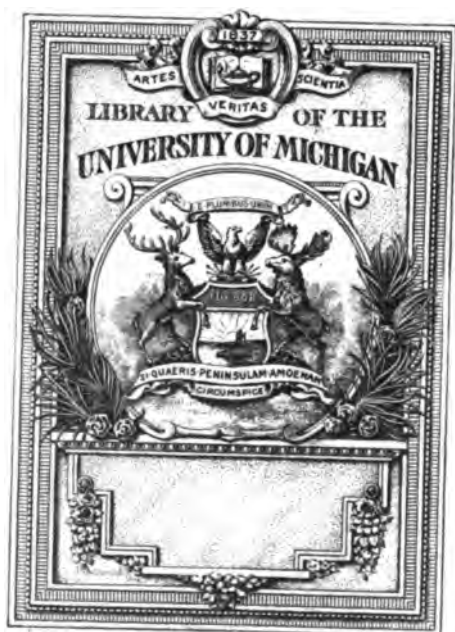
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# PROCEEDINGS



OF THE

## ROYAL SOCIETY OF LONDON.

*From November 30, 1860, to February 27, 1862 inclusive.*

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## OBITUARY NOTICES OF DECEASED FELLOWS.

SIR CHARLES BARRY was born on the 23rd of May, 1795, and was the son of a respectable stationer in Bridge Street, Westminster. Having displayed an early and striking talent for drawing, he was articled at the age of sixteen to Messrs. Middleton and Bailey, surveyors in Lambeth, with whom he remained five years, acquiring the elementary principles of his future profession, and those practical technicalities in which they were more particularly engaged; and he was occasionally a contributor to the Exhibition of the Royal Academy.

In 1816 he lost his father, and having succeeded to a few hundred pounds, he determined to go abroad in order to perfect himself in the highest walks of his art. He there studied the masterpieces of ancient and modern architecture existing on the continent, making a most diligent use of his time; for he was untiring in his application, and conscious of the great fruits to be derived from availing himself of every opportunity of laying up a rich treasury of reference for an after time. He spent several months at Rome; and with some other brother artists of like tastes, as Sir Charles Eastlake, the present President of the Royal Academy, and Mr. Kinnaird, Editor of the most recent edition of Stuart's 'Athens,' he passed on to Greece, where he studied the Parthenon, Theseum, and other monuments of the Periclean age, views of which he subsequently exhibited in the Royal Academy.

Having exhausted his means, he was on the point of returning home, when an English traveller, Mr. Baillie, who was struck by the freedom of his pencilling, and who wished to preserve some records of a projected tour in Egypt and Syria, engaged young Barry to accompany him as his artist; and during 1818 and 1819 he had the opportunity of drawing the remarkable monuments and sites of those interesting countries. In 1820 he returned to England, married, and entered upon the practice of his profession. He had for some years to struggle with the difficulties which usually fall to the lot of young men commencing a professional career, even those endowed with the highest qualifications, until they can procure opportunities of showing the extent of their acquirements and capacity for the actual

business of life. His earlier works of note were St. Peter's Church at Brighton and the Manchester Royal Institution, which he gained in competition. These at once gave him a name. He then built three churches in Islington; and having thus established his standing in the profession, he became very largely employed. The Traveller's Club House, which he also gained in competition, the Free Grammar School at Birmingham, and the Reform Club, are remarkable specimens of the elegance and vigour of his taste in various styles of art. But the great monument of the present age is the Palace of Westminster, or Houses of Parliament, the prize for which he carried off from nearly 100 competitors. This was in 1836; but it was not till 1840 that the works were actually commenced; and considering the immense extent of the building, the elaborate nature of its decoration, the variety of uses to which the several portions were to be applied, the accommodation to be provided, and the questions of light, ventilation, and selection of materials which demanded the laborious and incessant attention of the architect upon this, the largest of modern edifices in Europe, we must feel surprised that he was able to go through the harassing and arduous duties, and to complete it in twenty years. We may quote the opinion, and words almost, of his able biographer, Mr. Digby Wyatt, and say that "no public building of Europe possesses a more ingenious or effective plan, a more perfect homogeneity of parts and style, a more graceful outline under every point of view, and greater technical excellences and beauty. In vain will the most critical observer wander into every nook and corner of that enormous structure and detect a neglected point where the failing attention or ability of the architect has allowed a degeneration into meanness of finish, dissymmetry of axes or leading features, or faulty proportion of line or detail."

It would constitute a long list to enumerate the buildings of more or less importance in the metropolis and the provinces of which Barry was the architect. He was a Royal Academician, and on the occasion of the opening of the Houses of Parliament, he received the honour of knighthood from Her Majesty. To him also were awarded the great Gold Medal of the Paris Exhibition of 1855, and the Royal Gold Medal of the Institution of British Architects. He died somewhat suddenly on the 12th of May, 1860, at the age of 64, and

he was buried in the nave of Westminster Abbey, near the grave of his friend, Robert Stephenson, with all the public honours due to his genius and worth.

Barry was a man of rich and varied imagination, and so difficult to please, that he incessantly studied modification upon modification during the progress of his works. He had great power of drawing, and a thorough appreciation of form; and so perfectly master was he of both Italian and Mediæval art, which have elements of proportion, form, and decoration apparently at utter variance with each other, that he treated them with equal judgment and taste. But it was remarked that he had not a harmonious eye for colour. He was untiringly laborious, and as a practical architect he brought to bear a vast fund of scientific resource and daring intelligence. He was justly held in high professional estimation by the architects of all countries, and was honoured by all who knew him for his worth, integrity, and independence; and his name will descend to posterity associated with the grandest and finest edifices of his time.

SIR THOMAS MACDOUGALL BRISBANE, Baronet, was the eldest son and representative of an old family of considerable note in the west of Scotland. He was born at Brisbane House, the family seat in Ayrshire, on the 23rd of July, 1773, and after a long, active, and eventful life passed in various parts of the world, died at the same place on the 27th of January, 1860.

As was usual with the sons of the landed gentry in Scotland, Sir Thomas received his early education at home under a private tutor, and then studied for a time at the University of Edinburgh. He was finally sent to an Academy near London, where he had the opportunity of attending lectures in the metropolis on various subjects, including mathematics and astronomy.

In 1789 he began military life as an ensign in the 38th Regiment, but having, on the breaking out of the war in 1793, raised an independent company which was attached to the 53rd Regiment, he joined the army in Flanders as captain, and was present at the engagement before Valenciennes and at the siege and capture of that place, which were the first operations of the war. He shared in the severe winter's retreat through Holland in 1794, and continued with the Duke of York's army until its return to England in 1795. He

then sailed with the expedition under Sir Ralph Abercromby to the West Indies, where he remained for five years, and was at the reduction of St. Lucia and St. Vincent, the capture of Trinidad, and the attack on Porto Rico. He also served throughout the whole Caribbean war, and then went to Jamaica as Lieutenant-Colonel of the 69th, where he remained till his regiment was called home.

Having in 1812 received the appointment of Brigadier-General under the Duke of Wellington, Sir Thomas immediately proceeded to the Peninsula. In the course of his services there he commanded a brigade in five general actions, besides a great many minor affairs; and he received the thanks of Parliament for the part he took in the battle of Orthes, where he had 700 men of his brigade killed and wounded. At the termination of the war he accompanied a portion of the Duke of Wellington's army to North America and served in the campaign of 1814, but returned to Europe the year following with twelve regiments to join the army of occupation in France, where he remained during the three years of its stay in that country. A year or two afterwards Sir Thomas was appointed Governor of New South Wales, and entered on the duties of his office in 1821. During the four years of his government he made important ameliorations in the management of the convicts, turning their previously profitless labour to account in the clearance of the land and the service of the colonists, as well as to their own advantage; he introduced the cultivation of the vine, the sugar-cane, cotton, tea, and tobacco, and improved the breed of horses, by importations at his own expense from Mocha and Calcutta. On his departure for Europe, he received the grateful acknowledgements of the colony, and a prosperous district of that interesting region now bears his name.

For these eminent services, military and civil, Sir Thomas received the usual honours. He was created a Baronet and a Knight Grand Cross of the Bath, and finally rose to the rank of General. But his military career, distinguished as it was, and his wise and beneficent colonial administration, are matters of less immediate interest here than the services he has rendered to science, and especially to astronomy.

From his schoolboy days Sir Thomas was devoted to mathematical studies. To his favourite pursuit of astronomy, he was turned, as he himself states, by exposure to imminent danger in one of his early

voyages through a mistake of the commander of the ship in estimating the longitude; an incident which left on his mind a deep sense of the practical advantages of a knowledge of astronomy. His warm attachment to the science which thus arose, never abated, and he found means to indulge it even amid the distractions of an active and adventurous military life, by making observations with a pocket sextant; and we have the testimony of the Duke of Wellington that he kept the time of the army. In the same spirit he availed himself of a brief interval during which he returned home in 1808, to erect an observatory at Brisbane, his native place, in which he personally worked as an observer. But it was when appointed Governor of New South Wales that he was enabled to perform his greatest service to Astronomy. With this object in view, he carried with him to the colony an outfit of costly instruments, and immediately on his arrival in 1821, selected the site for an observatory at Paramatta, and set about the erection of a suitable building; all at his own expense. Before leaving England he had engaged the services of Mr. C. Rumker, then an astronomer of considerable experience, and now one of our Foreign Members, to conduct the contemplated observatory. His own public duties would of necessity seldom admit of his devoting any material portion of his time to actual observation, yet he frequently took a personal share in the work; and in particular, a great portion of the transits were observed by himself.

On the return of Sir Thomas and Mr. Rumker to England, a volume of the Observations made at Paramatta by Mr. Rumker and reduced by him were, with Sir T. Brisbane's approval, presented by Mr. Rumker to the Royal Society, and constitute Part 3 of the Philosophical Transactions for 1829.

It was through the labours at this observatory that astronomical science became enriched with the large series of observations of Southern Stars, for the most part communicated in the first instance to the Royal Society, but afterwards collected and published in 1835, under the well-known title of "The Brisbane Catalogue of Stars;" and on account of these observations, and the great services rendered by him to Astronomy in the Southern Hemisphere, Sir Thomas received the Gold Medal of the Royal Astronomical Society in 1828. In announcing this award, the President, Sir John Herschel, refers also to the observation of the solstices of 1821, 1822 and 1823; the rediscovery of



Encke's Comet in its predicted place on the 2nd of June, 1822, when, owing to its position in the heavens at its perihelion passage, it could not be seen in Europe; and the determination of the length of the pendulum at Paramatta. Speaking of the observations of the places of Southern Stars, the same pre-eminent authority characterizes them as forming one of the most interesting and important series which has ever been made, and considers that they must ever be regarded as marking a decided era in the history of southern astronomy; and he concludes his address in the following words:—"It is for this long catalogue of observations, whether scattered through the journals of Europe, printed in our own memoirs, or deposited as a precious charge in the care of a Body so capable of appreciating their merits, but still more for the noble and disinterested example set by him in the establishment of an Observatory on such a scale in so distant a station, and which would have equally merited the present notice had every observation perished on its voyage home, that your Council have thought Sir Thomas M. Brisbane deserving of the distinction of a medal of this Society." It may be added that this distinction was not the less deserved, although it has been since found that from causes which Sir Thomas could not well have obviated, the Catalogue of Stars has turned out to be a less valuable result of the Australian Observatory than its enlightened and munificent founder had reason to anticipate.

On his return from New South Wales, Sir Thomas founded an Astronomical Observatory at Makerstoun, a residence he possessed near Kelso; and he subsequently established at the same place a Magnetical Observatory, which he furnished with the best instruments, and appointed with a staff of able assistants, at his own charge. The magnetical observations made at this observatory since its erection in 1841, up to 1849, have been published in three quarto volumes, and a fourth, containing the later observations, is now in the press. The copyright of these volumes has been presented to the Royal Society of Edinburgh, and they now form part of the record of the Society's Transactions. For these magnetical observations, the Society awarded Sir Thomas the Keith Medal in 1848.

Sir Thomas Brisbane became a Fellow of the Royal Society in 1810; he was also a Fellow and Vice-President of the Royal Astronomical Society. He entered the Royal Society of Edinburgh as a

Fellow in 1811, and in 1832 was unanimously elected President as successor to Sir Walter Scott. During his presidency he founded two medals for the reward of scientific merit, one to be conferred by the Royal Society of Edinburgh, the other by the Scottish Society of Arts.

A list of Sir Thomas Brisbane's published contributions to science, drawn up by his friend Alexander Bryson, Esq., F.R.S.E., will be found in a memoir entitled "*Reminiscences of General Sir T. M. Brisbane,*" printed for private circulation, 4to, Edinburgh, 1860, to which we have been chiefly indebted in compiling the present notice.

Lieutenant-Colonel WILLIAM MARTIN LEAKE, born in London, January 1777, was the son of John Martin Leake, a commissioner for auditing the public accounts, and grandson of Stephen Martin Leake, Garter Principal King-at-Arms. He was educated at the Royal Military Academy, Woolwich, and in 1794 obtained a commission in the Artillery, and entered on the active duties of his profession in the West Indies. Five years later he was sent to Constantinople on a mission for the instruction of the Turks in the use of artillery; and in 1800 he was one of the English Officers selected to advise and assist the Grand Vizier in his defence of the southern provinces of the Turkish Empire against the French. Captain Leake consequently travelled through Asia Minor, and, after some delay, ultimately joined the Turkish army in Syria. In 1801 he crossed the Desert with the army into Egypt, and on the surrender of Alexandria he was appointed to visit Upper Egypt in company with Mr. William Hamilton, for the purpose of making a general survey of that country. The results of their exploration were a map of the course of the Nile, from the Cataracts to the sea; a determination of most of the ancient sites; a description of the monuments of antiquity, and a large collection of observations on the agricultural and commercial state of the country. An account of their travels was published by Mr. Hamilton in 1809.

Having returned to Syria in 1802, Capt. Leake there continued his travelling researches until the time of his departure for England. He embarked on board the vessel in which the Elgin marbles were placed for conveyance to London, and narrowly escaped with his life from shipwreck off the Island of Cerigo. In 1804 he received orders to survey the coasts and interior of European Turkey, the

fortresses and means of defence, and to make their condition known to the native chiefs and governors. In pursuance of these orders he travelled during two years in Northern Greece and the Morea, and discharged his duties to the full satisfaction of the home authorities, while, as has been remarked, "his peculiar tastes and talents for research received full development in a country where every day's journey produced an historical problem, which it taxed his erudition and critical acumen to solve."

On the breaking out of hostilities between England and the Porte in 1807, Capt. Leake was detained and held prisoner by the authorities at Salonica, from which place he, however, escaped to the frigate 'Thetis,' and returned to England for the restoration of his health in 1808.

From October of the same year until 1810 he again travelled in the Levant, under command to open communications with Ali Pasha of Joannina, and other authorities of the Ottoman government, with a view to induce them to proceed to offensive operations against the French, by offers of naval assistance along the coast, and presents of ordnance and ammunition. To these successive years of travel he owed his knowledge of the various countries, their people and monuments, which afterwards proved so valuable and instructive in his literary labours.

These labours were resumed on his retirement from the army in 1823, and were continued to the time of his death on the 6th of January, 1860. His works comprise 'Researches in Greece,' 'Topography of Athens,' an edition of 'Burckhardt's Travels,' 'Travels in Asia Minor,' 'Historical Outline of the Greek Revolution,' 'Demi of Attica,' besides other books of travels in Greece, and on the topography and coins of that country,—a country for which he always felt the liveliest interest. His sympathy for Greek nationality gained him the gratitude of the people, as manifested by the voluntary attendance of the Greek minister at his funeral.

In 1838 Col. Leake married Mrs. Marsden, widow of William Marsden, Esq., F.R.S., the historian of Sumatra, and daughter of the eminent oriental scholar Sir Charles Wilkins, F.R.S.

Col. Leake was a member of various learned societies, English and foreign. He became a Fellow of the Royal Society in 1815, and was elected a member of the Royal Society Club in 1828. His intellec-

tual vigour remained unabated to the close of a life prolonged to the age of 83, and its results are left to us in works which "are models of painstaking observation, available erudition, and perspicuous simplicity."

**JOSEPH LOCKE** was born at Attercliffe, near Sheffield, in 1806, and educated at the Grammar School of Barnsley, whence he passed to Newcastle-on-Tyne, and there learned the elements of engineering under George Stephenson. He commenced the active duties of his profession on the Liverpool and Manchester Railway, where during four years he had full opportunity for applying principles to practice, in the overcoming of 'engineering difficulties.' And he was one of those who, after the opening of the line in 1830, aided in demonstrating the superiority of the locomotive engine over the other kinds of motive power then proposed. He next completed the Grand Junction line connecting the Liverpool and Manchester Railway with Birmingham, which had been begun by George Stephenson, and established a reputation for economy which he always afterwards retained, by keeping the cost within the estimate. To this proof that a railway could be made for less than £15,000 a mile, and to his habitually cautious methods, he owed most of his success. His other principal works are the London and Southampton Railway; the Havre and Paris Railway, *via* Rouen; the line from Barcelona to Mattaro, and the Dutch-Rhenish line. Moreover, the Lancaster and Carlisle, the East Lancashire, the Caledonian, the Scottish Central and Midland, the Aberdeen lines and Greenock Railway and Docks, the Paris and Cherbourg line, were all constructed under his superintendence, jointly with his partner, Mr. Errington. He planned lines also which were constructed by other engineers.

Few men have excelled Joseph Locke in resolute adaptation of means to ends; even his greatest works display no signs of extravagance, and yet are efficient, and appropriate to their situation. He had no unimportant share in the improvement of the locomotive, and showed the practicability of making it travel over unusually steep gradients.

Mr. Locke succeeded Robert Stephenson as President of the Institution of Civil Engineers; he was elected a Fellow of the Royal Society in 1838, and from the year 1847, to the time of his decease,

he sat in Parliament for Honiton. He was suddenly and prematurely cut off by an attack of iliac passion at Moffat, on the 18th of September, 1860.

CHARLES MAY, son of a member of the Society of Friends, was born at Alton, Hampshire, in the year 1800. At an early age he began to exhibit proofs of the mechanical genius which became conspicuous in his later years, and with but little help from schooling, for his education is said to have been scarcely better than that now within reach of workmen. He was apprenticed to Mr. Sims, a chemist and druggist of Stockport, whose daughter he afterwards married; and having returned to Ampthill, to which town his parents had removed from Alton, he commenced business on his own account, as a retail and manufacturing chemist, and to this in course of time he added the business of a millwright.

In 1836 he accepted a partnership with Messrs. Ransome, agricultural implement makers of Ipswich, taking charge of the engineering department, and the results of his vigorous management were speedily shown by the rising importance of the firm and increase of business. During this connexion Mr. May constructed a dome for the observatory at Hartwell House, and for Mr. Barclay's observatory at Bury Hill, the equatoreal mounting for Ross' large object-glass, which occupied a conspicuous place in the Exhibition of 1851, and the great Transit Circle and the Altazimuth, now in use at the Greenwich Observatory. The latter are deservedly admired for their perfect construction, which completely realized the conceptions of the Astronomer Royal, and improved to an unprecedented extent the mechanical resources of astronomical science. Mr. May moreover erected an observatory for his own use, and furnished it with a transit instrument and clock, and a Merz achromatic telescope of  $6\frac{1}{2}$  inches aperture. And in other departments of the engineering business he instituted experiments on the strength of iron, effected improvements in the permanent way of railways, invented the compressed tree-nails for fixing railway chairs to the sleepers, and was the first to introduce the process of 'chilling' for the pivots of large instruments.

Mr. May was a member of the Royal Astronomical Society and of the Institution of Civil Engineers; he was elected into the Royal

Society in 1855. He removed from Ipswich to London in 1851, and died suddenly on the 10th of August, 1860, leaving behind him a reputation for high professional ability, and for an amiable and beneficent disposition, varied by keen humour, in private life.

The biography of Lord Macaulay belongs rather to the history of Literature than to that of Natural Philosophy: he takes his proper place among the Statesmen, Orators, Poets, Essayists, Historians of England, not among her men of Science. With a mind so active and wide-ranging, he could not but take deep interest in the progress and in the marvellous discoveries of modern science; but he was content to accept those results on the authority of others, and to dwell on their political and social consequences, rather than himself to follow out their slow and laborious processes, for which, indefatigable as he was, he had no time, probably no inclination. Yet the annals of the Royal Society, which has ever been proud to enrol among its Members statesmen and men of letters of the highest eminence, cannot pass over in silence a name so illustrious as that of Lord Macaulay.

THOMAS BABINGTON MACAULAY was born October 25, 1800, at Rothley Temple in Leicestershire, the seat of his paternal uncle Thomas Babington. His father, Zachary Macanlay, resided at Clapham, one of those earnest and zealous men who, with Mr. Wilberforce, led the way in the strong religious reaction which followed the French Revolution, and whom posterity will honour as among the earliest and most steady adversaries of the African Slave Trade, the advocates of the emancipation of the negroes in our Colonies. The perpetual agitation of such questions, involving the most sacred principles of human liberty, could not be without its effect on the precocious mind of the young Macaulay. Perhaps to his birth and training in that school he owed in some degree his command of biblical illustration, which, however, his strong sense and sober judgement always kept within the limits of serious and respectful reverence. Family traditions, happily only traditions, of his early promise, of his childish attempts at composition in prose and verse, were not likely to be lost among a strong religious party, bound together by common sympathies, and maintaining an active correspondence throughout the country. The fame of young Macaulay reached the ears of Hannah More, and, after receiving a visit from him, the High Priestess of the

brotherhood, in an agreeable letter, still extant, uttered an oracle predictive of his future greatness. After a few years of instruction at a small school in Clapham, at the age of 12 he was placed under the care of the Rev. Mr. Preston, first at Shelford, afterwards near Buntingford, in the neighbourhood of Cambridge. Mr. Preston seems to have been a man of attainments and judgement. He must have taught the Latin and Greek authors extremely well, for under his instruction Macaulay became a sound and good scholar. He did more, he fostered that love for the great classical writers, without which all study is barren and without durable impression. He respected too that great maxim, that no one is so well taught as by himself. Having given or strengthened the impulse, he left the young scholar to his own insatiable avidity for learning, and for books of all kinds. The schoolboy sent an anonymous defence of novel reading to the serious journal of his father's friends, the *Christian Observer*, which was inserted. This passion for novel reading adhered to him to the last; he swept the whole range, not only of English but of foreign fiction, not without great profit to the future historian. The higher tastes which he then imbibed were equally indelible; his admiration of the unrivalled writers of Greece and Rome grew deeper to the close of his life. Homer and Thucydides, and Tacitus, remained among his constant and familiar studies, and no doubt, without controlling him to servile imitation, exercised a powerful influence on his mode of composition and on his style. Among his father's friends holding the same religious opinions, was Isaac Milner, Dean of Carlisle, and Master of Queen's College, a man with a singular union of profound mathematical acquirements, strong evangelical views, and a peculiar broad humour. During his visits to Milner at Cambridge, Macaulay acquired that strong attachment to the University, which, like his other attachments, seemed to become more strong and fervent with the progress of years.

In his 19th year he began his residence at Trinity College, Cambridge. His career at Cambridge was not quite so brilliant as the sanguine expectations of his friends had foretold. He had a repugnance for mathematics, or rather he was under the jealous and absorbing spell of more congenial studies. That repugnance in after life was a subject of much regret; he fully recognized the importance, almost the necessity, of such studies for perfect educa-

tion; even his scholarship, probably far more extensive, wanted that exquisite polish and nicety acquired only at our great public schools, from which came his chief rivals. He carried away, however, the Craven Scholarship, two prizes for English verse, and finally, the object of his highest ambition, a Fellowship of Trinity College. On this success he dwelt to the close of his life with pride. It gratified two of his strongest feelings,—attachment to Cambridge, and the desire of some independent provision which should enable him to enter on his professional career. On the inestimable advantages of such fellowships to young men of high promise and ability but of scanty means, he always insisted with great earnestness, and deprecated any change in the academical system which should diminish the number of such foundations, held, as he would recount with his unflinching memory, by so many of our first public men.

The Law was the profession he chose; he was called to the Bar at Lincoln's Inn, February 1826; he took chambers, he read, he joined the Northern Circuit. But literature was too strong for law. His legal studies were no doubt of infinite value; they were in truth indispensable for his historical writings, and were hereafter to bear fruit in a sphere which his wildest imagination could not anticipate. He had received, indeed, from the discerning judgment of Lord Lyndhurst, a Commissionership of Bankrupts, 1827. No doubt his Cambridge fame and general promise recommended him for that office. But it was to letters that he was to owe his first opening to public life. In letters he had begun with modest contributions to a magazine, *Knight's Quarterly*, of no great circulation, but which was mainly supported by some of his Cambridge friends: in this appeared some of his finest ballads. On a sudden he broke out with an article on Milton in the *Edinburgh Review*, which perhaps excited greater attention than any article which had ever appeared, not immediately connected with the politics of the day. Taking the field in the same pages with the brilliant copiousness of Jeffrey, the vigorous and caustic versatility of Brougham, the inimitable wit and drollery and sound sense of Sydney Smith, to say nothing of the writers in the rival *Quarterly Journal*, the young reviewer had struck out his own path. In comprehensiveness of knowledge, in the originality and boldness of his views, in mastery over the whole history and the life of the eventful times of Milton, in variety and felicity of



illustration, in vigour, fulness, and vivacity of style, he seemed to make an epoch and a revolution in review-writing. Up to this time, with some excellent exceptions, the articles in reviews had confined themselves to notices, more or less excursive, of new books, and to discussions of the political or polemic questions of the day. The article now aspired to be a full dissertation on the history of any great period, on the life of any great man of any time, on the writings, on the influence, on the merits of authors of the highest fame. From a review it became an historical, biographical, philosophical essay.

This paper was followed by others of equal, some perhaps of superior excellence, each opening a new view into the vast range of the author's reading, showing his boldness and independence of judgement, the wonderful stores of his memory, his prodigality, sometimes perhaps uncontrolled, of allusion, illustration, similitude. A young Whig, of high and blameless character, popular with his friends, with the reputation of oratorical power in the Debating Rooms at Cambridge (he delivered one speech in London, we believe, at an Anti-Slavery Meeting, which made some noise), and the acknowledged author of such articles in one of the two popular journals of the day, could not but command the attention, and awaken the hopes of his party. If ever there was a nobleman a patron of letters from a deep and genuine and discriminating love of letters, it was Lord Lansdowne. Lord Lansdowne offered a seat in Parliament to the author of the admirable articles in the Edinburgh Review. On the acceptance of this offer there could be no hesitation; his political opinions were in the strictest unison with Lord Lansdowne's. Few public men have been so calmly, deliberately true to their first political opinions as Macaulay. Unquestionably, change of political opinions, on full unselfish conviction, according to change of circumstances, may be the noblest act of moral courage, especially in the face of obloquy and misrepresentation. The best men may become wiser as they grow older. But to this trial Macaulay was never subjected, he was never called upon to this effort of self-sacrifice. He was a liberal in the highest and widest sense; some may think that he carried these views too far, some not far enough. But during life he was unswerving, without vacillation. The line which he drew between constitutional liberty and democracy in his early speeches on Reform and on the Charter, was precisely the same with that which he drew

late in life, in a remarkable letter on the prospects and probable destiny of the United States of America.

Four years after he had been called to the bar, in 1830, Macaulay was returned to Parliament for Calne. His public life had now commenced. That public life it may be convenient briefly to survey in its several phases, as Statesman, Orator, Poet, Essayist, Historian. Such was his remarkable variety and versatility. Very few men indeed have achieved great things in such different kinds of excellence.

In Parliament he had too much wisdom, too much self-respect, too much respect for his auditory (an auditory just in the main but severe, sometimes capricious in its justice, and jealous above all even of merit, if obtrusive, importunate, or too self-confident), to thrust himself forward at once into the foremost ranks. Till the Reform Bill he was content to try his arms on rare occasions; he would not waste his power on desultory skirmishes and on trivial subjects. Upon that momentous question, the Reform of 1832, he first put forth his strength. But of his speeches hereafter. The reputation acquired during these debates secured him a seat in Parliament, independent even on generous and unexacting friendship; he was returned, December 1832, for the wealthy and populous borough of Leeds, enfranchised by the Reform Bill. In the year 1834, a great, and no doubt unexpected change took place in his prospects, it might seem in his destination. In 1832 he had accepted the office of Secretary to the Board of Control. In his official capacity (in 1834) he made a speech on the renewal of the Indian Charter, a speech which may be read in no unfavourable comparison with Burke's most splendid orations. In breadth and comprehensiveness of view it may compete, in fulness and accuracy surpass, in richness of diction rival the renowned orator; of course, as the occasion was so different, it had nothing of the passion, the terrible picturesqueness, the vituperation; but it had calm statesmanship, and philosophical, or rather, perhaps, historical thought. This speech of itself might seem to designate him to the Government as a member of the New Council which was to legislate for India. The offer was made. The vast field of India was of itself likely to seize on his imagination; he might aspire to be the legislator, as Heber the religious missionary, of that wonderful realm. He had many friends, the family of Grant especially (the present Lord Glenelg was the President of the Board of Control), closely

connected with India ; how much he had read or thought on the subject, his papers on Clive and Hastings (written later) may nevertheless bear testimony. Still, no doubt, prudential motives, and those of no ungenerous prudence, influenced his determination. By a few years of economy, careful but not illiberal, he might make a provision for his future life (he was a man with no expensive or prodigal habits) which might place him above dependence either on the servitude of office, or the servitude of literary labour. There was another incentive—his family had never been affluent. He might add to the comforts and assist in the advancement of those to whom he was attached by the strongest domestic affections, a duty which he discharged with unsparing generosity. In India he took his seat as Member of the Council and as President of the Law Commission. It has been supposed, and indeed asserted, that this legislative mission was barren and without result ; now, however, it is bearing its mature fruits. After much, perhaps inevitable, delay, and repeated revisions, the Indian Criminal Code, in the formation of which he took a leading part, and which he had enriched with most valuable explanatory notes, will, with some alterations, and those not substantial, from January next have the force of law throughout British India. Macaulay's share in this great work, especially his notes, is declared by those who have a right to judge on such subjects, to have placed his reputation as a jurist on a solid foundation. It is the first, and therefore the most important, of a series of operations upon the judicial system of India, which will have a great effect upon the state of society in that country ; and will not be without influence upon the jurisprudence of England.

Soon after his return to England in 1838, in January 1840, he was elected by acclamation, representative of the City of Edinburgh ; that seat he filled undisturbed till July 1847. He had already been named on the Privy Council, and had accepted the office of Secretary at War. He was Secretary at War, with a seat in the cabinet, about two years, from 1839 to 1841. On the return of his friends to power, he became, July 12, 1846, Paymaster of the Forces.

But throughout this period of his life the great inward struggle was going on within his mind between the ambition of public usefulness, of parliamentary and official distinction, and the love of letters, which will rarely brook a rival on the throne, the still higher ambition,

as he thought, of adding some great work to the treasures of English thought and English literature. In the office at Whitehall or the Horse Guards, on the benches of the House of Commons, amid the applauses or admiring silence of the House, his heart was in his library, and among his books. He yearned for a place not so much among the great parliamentary leaders and the famous statesmen of the land, the Chathams, Burkes, Foxs, as among the immortal writers in verse and prose, the Miltons, Clarendons, Addisons, Gibbons. The auditory which he coveted was that vast expanding world throughout which the English language is spoken; the fame, that which will only die with the death of English letters. Throughout the whole time of his absence from England, on his voyage to India and on his return, in India, as far as leisure would allow, and during his parliamentary and official career, he was still with his indefatigable industry heaping up stores of knowledge, stores which could not overload his capacious and retentive memory,—memory, whose grasp and self-command seemed to expand with its accumulating treasures,—memory, which disdained nothing as beneath it, and was never perplexed or burdened by its incalculable possessions. As a curious instance of his range and activity of reading, among the books which he took with him to India, were the many huge volumes of St. Chrysostom's works. Their still almost pure and harmonious Greek, and their importance in the history of religious opinion (always a subject of deep interest), carried him through a task which has been achieved by few professional theologians. As an illustration of his powers of memory, he has said, and he was a most unboastful man, that if Milton's great poem were lost, he thought that he could accurately commit to writing at least all the first books of *Paradise Lost*.

This life-long inward strife, which perhaps might have remained unreconciled till towards the close of his days, came to a sudden and unexpected issue. At the election in 1847, Macaulay was the rejected candidate for the City of Edinburgh. Nor can it be denied, though those who admire Macaulay will not admire him the less, that he was accessory to his own failure. The event turned on a religious question, in which Edinburgh, true to its old Scotch prejudices, adhered to the less liberal view. Macaulay could not be persuaded to humour, to temporize, even to conciliate. He took the

loftiest tone, boldly, indignantly rebuked the voters for their narrow, in his estimation, discreditable bigotry. He felt, there can be no doubt, this blow at the time bitterly. He was perhaps not suited for, he had never before been tried in the rough and coarse work of the popular canvass and the hustings; he was distressed at the desertion or the lukewarmness of friends; he was ashamed, as he openly declared, of the disgrace which Edinburgh inflicted on herself. In a striking poem, recently published, in which are some of the finest stanzas in the language, he gave full vent to his feelings of indignation and sorrow. But at the same time, and in the same poem, he finds and expresses his lofty sense of consolation. The great debate was ended; he was released; he was emancipated from public, from parliamentary life. He might retire with dignity and honour to the undisturbed, undistracted cultivation of letters; henceforth his study was his scene of action; literary fame was to be the undivided mistress of his affections, his earthly exceeding great reward. Edinburgh made a few years after noble amends by returning Macaulay (at the election in 1852) without solicitation, without expense, even without the usual flattery of a personal canvass; he had but to appear, to accept, and return thanks for his ovation. He sat for Edinburgh from July 1852 to 1856. But he sat without the trammels, without the least desire of office: he spoke rarely, but never without effect. In 1856, failing health compelled him to resign that honourable post. Some other honours, but honours which belonged to a man of letters, awaited him and courted his acceptance. He was Lord Rector of the University of Glasgow in 1848; Trustee of the British Museum, February 1847 (an office which he highly esteemed, and to which he attended with much assiduity, and with great public advantage); Fellow of the Royal Society, November 1849; Foreign Member of the French Academy, May 1857, and of the Prussian Order of Merit (1857); High Steward of Cambridge (1857). In the same year he was raised to the peerage, a tribute to his high and blameless character and transcendent literary distinction, and an act of royal favour, quite unexpected, but highly approved by all whose approbation was of real value.

So far our imperfect sketch has exhibited Lord Macaulay as a public man, as a jurist, and as a statesman; some words must follow as to his rank as an orator. It is remarkable how rarely in this

country the famous and commanding public speaker, either in parliament or even at the bar, and the great writer, have met in the same person. Bolingbroke, Burke, and Macaulay (the unrivalled comedies of Sheridan, the State Papers and exquisite political satires of Canning are hardly in point) stand perhaps alone. If all the writings of Chatham, Pitt, Fox, Erskine, Peel, had been suppressed, the world would have suffered no great loss. Macaulay had no thought of resting his fame on his parliamentary speeches; he would willingly have left them to the rarely visited cemetery of the parliamentary history. He was placed under compulsion by the act of a piratical bookseller, who printed many of them (insinuating that he did so by authority) bristling with blunders, bad English, loose argument, errors and mistakes about events and persons, everything most abhorrent to Macaulay's taste and judgement. He was under the necessity of publishing a more trustworthy edition. We confess some gratitude for this bad act of the unprincipled Curll of our days, for some of these speeches appear to us oratorical compositions of the highest order. By all accounts Macaulay's delivery was far too rapid to be impressive; it wanted also variety and flexibility of intonation. Even the most practised reporters panted after him in vain; how much more the slower intellects of country gentlemen and the mass of the House! This, however, only heightens our astonishment that speeches so full, so profoundly meditated, yet with so much freedom, with no appearance of being got by heart, with such prodigality of illustration and allusion, should be poured forth with such unhesitating flow, with such bewildering quickness of utterance. To read them with delight and profit, we read them rather slowly; we can hardly conceive that they were spoken less deliberately. It may be questioned, and has been questioned, whether Macaulay was, or could have become, a masterly debater. This accomplishment, except in rare examples, is acquired only by long use and practice. When Macaulay entered the House, the first places were filled by men of established influence and much parliamentary training. Even if he had felt called upon to make himself more prominent, it may be doubted whether he could have sufficiently curbed his impetuous energy, or checked his torrent of words. He would have found it difficult to assume the stately, prudent, reserved, compressed reply; he might have torn his adversaries' arguments to shreds, but he

would not have been content without a host of other arguments, and so would have destroyed the effect of his own confutation. Still it is remarkable that on two occasions a speech of Macaulay's actually turned the vote of the house, and carried the question (a very rare event) in his own way,—the debate on the Copyright Act, and the question of Judges holding seats in the House of Commons. Though he took his seat, Lord Macaulay never spoke in the House of Peers ; he went down, we believe, more than once, with the intention of speaking, but some unexpected turn in the debate deprived him of his opportunity ; his friends, who knew the feeble state of his health at that time, were almost rejoiced at their disappointment in not hearing him in that which would have been so congenial a field for his studied and matured eloquence.

As a poet, the fame of Macaulay rests, with the exception of the stanzas above alluded to, and one or two small pieces, on his Ballads, his 'Lays of Rome,' his 'Armada,' his 'Cavalier' and 'Cromwellian,' and his 'Ivry and Moncontour.' In other departments of poetry he might have been endangered by his affluence and prodigality ; his prize poems, and some of his early writings betray the danger. But the essence of the ballad, of popular poetry (for which in all its forms, from the Prince of ballad writers Homer, to the common street ballad, which he caught up instantaneously, and could repeat by the score, he had an absolute passion), is simplicity—simplicity not inconsistent with the utmost picturesqueness, with the richest word-painting. Its whole excellence is in rapidity of movement, short sudden transition, sharp emphatic touches of tenderness or of the pathetic, in above all, life, unrepenting, unflagging, vigorous, stirring life, with words enough, but not an idle word, words which strike home to the heart, and rivet themselves on the memory ; a cadence which enthralls and will not die away from the ear. The popularity of Macaulay's ballads is the best proof of their excellence ; they have become the burden of a host of imitators. Popularity may be a bad test of some of the higher kinds of poetry. Dante, Milton, Shakespeare, to be fully appreciated, may require a thoughtful, refined, enlightened constituency ; ballad poetry may be safely left to universal suffrage.

Even in his famous Essays Macaulay had not satisfied his own ambition, nor reached that place after which he aspired in English

letters. He seemed disposed to leave them buried in the voluminous journal in which they had appeared. Here, however, it was the honest admiration of the public, not the base desire of a bookseller for gain, which suggested and indeed compelled their separate publication. America set the example: the first collection was made to gratify the laudable curiosity of those who are spreading our language and our literature over a continent to which our island is but a speck in the ocean. However flattering this homage, American editions are not to be implicitly depended upon, and are confined to their own use. It became necessary to answer the demand in England, and edition after edition has followed in rapid unexhausted succession. On these essays (not perhaps fitly so called, at least very unlike the short essays on religious, moral, social subjects, such as Bacon's, Cowley's, Addison's, Johnson's, Goldsmith's) we cannot of course speak at length. They are rather philosophical, or historical disquisitions, and are remarkable in the first place for their vast range and variety. Some grapple with the most profound questions,—the Baconian philosophy, the law of population against Mr. Sadler, and what is called the Utilitarian philosophy. This essay Macaulay himself, with noble moderation and self-respect, refused to include in his own selection, not because he was disposed to retract one argument, or to recede from the severity of his judgement on the opinions which he undertook to refute, but because he had not done justice to the high character of his adversary, the late Mr. Mill. Some belong to literary criticism, on which he delighted to mingle singularly acute and original observations on the biographies of distinguished authors, their place in society; and the articles on Dryden, the Comic Dramatists of Charles II., Temple, Addison, Johnson, Byron, are the most full, instructive, and amusing views of the literary life of their respective ages, as well as of their specific works. The greater number, however, and doubtless the most valuable of the essays, are those which belong to history; a few to the history of Europe,—Machiavelli, Ranke's Lives of the Popes, Frederick the Great, Mirabeau, Barrère. In these two last, his judgements on the acts and on the men of the French Revolution are very striking. But the chief and the most important are those on English History. This was manifestly the subject which he had thought on most profoundly, investigated with the greatest industry, and studied down to what we may



call the very dregs and lees of our political and social and religious life. There is hardly an important period, at least in our later history, which has not passed under his review. With the justly honoured exception of Hallam's *Constitutional History*, Macaulay usually dismisses his author with a few words of respect or contempt, and draws almost altogether on his own resources. So Burleigh gives us the reign of Elizabeth; Bacon that of James I.; Milton, Hampden, of Charles I. and the Republic; Temple (with Mackintosh's *History*), Charles II. and the Revolution; Horace Walpole, Chatham, Pitt, the Georges; Clive and Hastings, the rise of our Indian Empire. The variety of topics is almost as nothing to the variety of information on every topic; he seemed to have read everything, and to recollect all that he had read.

As to the style of these essays, of Macaulay's style in general, a few observations. It was eminently his own, but his own not by strange words, or strange collocation of words, by phrases of perpetual occurrence, or the straining after original and striking terms of expression. Its characteristics were vigour and animation, copiousness, clearness, above all, sound English, now a rare excellence. The vigour and life were unabating; perhaps in that conscious strength which cost no exertion he did not always gauge and measure the force of his own words. Those who studied the progress of his writing might perhaps see that the full stream, though it never stagnated, might at first overflow its banks; in later days it ran with a more direct undivided torrent. His copiousness had nothing tumid, diffuse, Asiatic; no ornament for the sake of ornament. As to its clearness, one may read a sentence of Macaulay twice, to judge of its full force, never to comprehend its meaning. His English was pure, both in idiom and in words, pure to fastidiousness; not that he discarded, or did not make free use of the plainest and most homely terms (he had a sovereign contempt for what is called the dignity of history, which would keep itself above the vulgar tongue), but every word must be genuine English, nothing that approached real vulgarity; nothing that had not the stamp of popular use, or the authority of sound English writers, nothing unfamiliar to the common ear.

The *Essays*, however, were but preparatory, subsidiary to the great history, which was the final aim, and the palmary ambition of Macau-

lay. On the function, on the proper rank, on the real province and use of history, he had meditated long and profoundly. His ideal of the perfect historian, such as he aspired to be, may be found in an essay, somewhat too excursive, in the *Edinburgh Review*, republished in the recent volumes. A perfect history, according to Macaulay, would combine the unity and order of the great classical historians, with the diversity and immense range of modern affairs. This was but one condition; the history would not be content with recording the wars and treaties, the revolutions and great constitutional changes, the lives of kings, statesmen, generals; it would embrace the manners, usages, social habits, letters, arts, the whole life of the nation. It would cease to be haughtily aristocratic; it would show the progress of the people in all its ranks and orders. There can be no doubt that, as to the actual life of certain periods, Shakespeare and Scott are more true and trustworthy historians than Hume or even Clarendon. Why should not romance surrender up the province which it had usurped? Why should not all this, which is after all the instructive, not to say amusing part of the annals of mankind, be set in a framework of historic truth, instead of a framework of fiction? If we would really know our ancestors, if we would really know mankind, and look to history for this knowledge, how can history, secluding itself in a kind of stately majesty, affect to disdain this most important part of her office? Nothing can be more clumsy than the devices to which the historian sometimes has recourse. It may be excusable in historic dissertations (the form which Hallam's works have assumed) to have the book half text, half notes,—broken, fragmentary, without continuity. Hume and Robertson took refuge in appendices, in which they sum up with unsatisfactory brevity, what they have wanted skill to inweave into their narrative. Henry's history may be read as containing what Hume left out. If there is in notes much beyond citation of authorities, perhaps comparison of conflicting authorities (we may perhaps pardon in Gibbon something more), this can only show that the historian has an unworthy conception of his high art, or that he wants the real power and skill of an historian. But to this lofty view of the historian's function who is equal? It required all Macaulay's indefatigable research; for the historian, the true historian, must not confine himself to the chronicles and annals, the public records, the state papers, the political correspondence of

statesmen and ambassadors; he must search into, he must make himself familiar with the lowest, the most ephemeral, the most contemptible of the writings of the day. There is no trash which he must not digest; nothing so dull and wearisome that he must not wade through. Nor are books all; much is to be learned from observation; and Macaulay delighted in rambling over England, to visit the scenes of historic events, the residences of remarkable men: the siege of Derry was described from Derry and its neighbourhood; the exquisitely true and vivid epithets with which he paints the old Italian towns in his Roman ballads owe their life and reality to his travels in Italy. Finally, to order, dispose, work into a flowing and uninterrupted narrative, the whole of this matter demanded nothing less than his prodigious memory, ever at the command of his imagination; to arrange it without confusion, to distribute it according to the laws of historic perspective, to make it, in short, a history, as difficult to lay down as the most stirring and engrossing romance.

Alas! that all this matchless power and skill should end in a torso,—yet a torso if, as we fairly may, we take the Revolution and the reign of William III. as a whole, nearly complete in its stature, and in all its limbs! It is deeply to be lamented that Macaulay allowed himself to be called off by generous and grateful friendship to write the lives in the *Encyclopædia*. All of these, even that of Pitt (as far as it goes, a perfect biography), we would willingly sacrifice if we could fill up the few chasms in his history. And what would we not give for his Queen Anne? William III., to whom he first did justice, and not more than justice, when looked upon from a European, not from an English point of view, was a labour of love: but what would have been the more congenial age of Anne, in which he knew every one, the Queen and her Court, Harley, St. John, Swift, Pope, Arbuthnot, as if he had lived with them on the most intimate terms? That in the main Macaulay possessed the still higher qualities of a historian, truth and impartiality, we hesitate not to avow our opinion; of this posterity will judge, we quietly and confidently await its award. He spoke out too freely, too strongly, not to encounter some prejudices, some no doubt very honest political or religious feelings. He did not perhaps always nicely measure the strength of his own language; and he so abhorred meanness and dishonesty, that they appeared doubly mean and dishonest in men

of great fame and high pretensions. As to Marlborough, we are content to place Mr. Hallam's even more condemnatory verdict by the side of Macaulay's; and Macaulay had not reached the brighter part of Marlborough's career; in the last volume that great man is already shaking off the slough of his baser life. Penn's double and conflicting character (assuredly no rare occurrence in history) must be viewed on all sides. In Pennsylvania, the wise, Christian legislator, worthy of all praise, he was, in England, a vain busy man, proud of his influence with the king, who found it his interest to flatter him, and unable to keep himself out of the miserable intrigues of that miserable court.

A few sentences on Macaulay's conversational powers, on his private life still fewer. There is a common impression that in society he was engrossing and overpowering. Every one has heard the witty saying of his old friend (no two men could appreciate each other more highly or more justly) about "flashes of silence." But in the quiet intercourse with the single friend, no great talker was more free, easy and genial, than Macaulay. There was the most equable interchange of thought; he listened with as much courtesy, as he spoke with gentle and pleasant persuasiveness. In a larger circle, such as he delighted to meet and assemble around him to the close of his life, a few chosen intimates, some accomplished ladies, foreigners of the highest distinction, who were eager to make his acquaintance, his manners were frank and open. In conversation in such a circle, a commanding voice, high animal spirits, unrivalled quickness of apprehension, a flow of language as rapid as inexhaustible, gave him perhaps a larger share, but a share which few were not delighted to yield up to him. His thoughts were like lightning, and clothed themselves at once in words. While other men were thinking what they should say, and how they should say it, Macaulay had said it all, and a great deal more. And the stores which his memory had at instantaneous command! A wide range of Greek and Latin history and literature, English, French, Italian, Spanish; of German he had not so full a stock, but he knew the best works of the best authors; Dutch he learned for the purpose of his History. With these came anecdote, touches of character, drollery, fun, excellent stories excellently told. The hearer often longed for Macaulay's memory to carry off what he heard in a single

morning, in an after-dinner colloquy, or in a few hours in a country house.

Lord Macaulay was never married ; his strong domestic affections were chiefly centred in his sister, happily married to his friend Sir Charles Trevelyan, and her family. Her children were to him as his own, and cherished with almost parental tenderness. As a friend, he was singularly steadfast ; he was impatient of anything disparaging of one for whom he entertained sincere esteem. In the war of political life, he made, we believe, no lasting enemy ; he secured the unswerving attachment of his political friends, to whom he had been unswervingly true. No act inconsistent with the highest honour and integrity was ever whispered against him. In all his writings, however his opinions, so strongly uttered, may have given offence to men of different sentiments, no sentence has been impeached as jarring against the loftiest principles of honour, justice, pure morality, rational religion.

In early life he was robust and active ; and though his friends at a later period could not but perceive the progress of some mysterious malady (he was long harassed by a distressing cough), yet he rallied so frequently, and seemed to have so much buoyancy of constitution, that they hoped he might have life to achieve his great work. He himself felt inward monitions ; his ambition receded from the hope of reaching the close of the first Brunswicks ; before his last illness he had reduced his plan to the reign of Queen Anne.

His end, though not without warning to those who watched him with friendship and affection, was sudden and singularly quiet ; on December 28, 1859, he fell asleep and woke not again.

He was buried, January 9, 1860, in Westminster Abbey, in Poet's Corner, his favourite haunt ; and he was known to have expressed a modest hope that he might be thought worthy to repose there with the illustrious dead. He lies at the foot of Addison's statue, near to Johnson, and among many other of our most famous statesmen and men of letters.

The following notice of Professor Powell is taken from the Report for 1861 of the Council of the Royal Astronomical Society, of which he was a distinguished member.

"The Rev. BADEN POWELL was the eldest son of the late

Rev. Baden Powell, of Langton in Kent, and was born at Stamford Hill, near London, in 1796. He graduated at Oxford, where he took a first-class degree in mathematics in 1817. In 1820 he entered into holy orders, and in the following year obtained the vicarage of Plumstead in Kent. He was appointed Savilian Professor of Geometry in 1827, and Public Examiner soon afterwards; a post with which he was again honoured in 1831. He thus stood, as it were, the acknowledged representative of mathematical and physical science in the elder English University,—we may almost say the only representative; for when he first obtained the professorship, science was at the lowest ebb at Oxford. Mr. Powell exerted himself to bring this state of things before the educated world, by which he incurred some obloquy. But he persevered, and succeeded; the honourable position which Oxford now holds, and the bright prospects of the future, are due in good part to the exposure and remonstrance of the Savilian Professor.

“He performed the duties of his chair with urbanity, seeking to encourage his students by explaining and familiarising each point that would allow of it; towards which he had a happy talent of constructing his own models, of the simplest materials, for his practical illustrations. Thus, whenever requested to deliver lectures at public institutions, he came armed with appropriate diagrams and forms that essentially aided the comprehension of those who crowded to hear him. We have thus seen the Undulatory theory of Light treated, and Diffraction made—we may say—palpable. The Precession of the Equinoxes, and the phenomena of Aberration and Nutation, were rendered equally visible; and the hurried progress of Comets when approaching the perihelion, was so attractively shown, that the audience flocked down, after the lecture, to watch the comet vary its speed through its long ellipse towards the sun. His style of delivery was peculiarly quiet, showing his own clear conviction and comprehension of his subject; and his calmness was caused by depth of thought. This sedateness of manner pervaded all his writings and discourses, enabling him even to preach extempore on the most intricate doctrinal points without wandering from his subject.

“Meanwhile he had produced various works, as well scientific as theological. Among the first class may be cited his ‘Treatise on the

Differential and Integral Calculus;' and another on the 'Geometry of Curves.' In 1834 he published his 'History of Natural Philosophy,' and in 1841 the 'Undulatory Theory of Light;' besides which he was the author of several papers in the 'Philosophical Transactions' on Light, Heat, and Irradiation. Moreover, his tracts appear in the records of this [the Astronomical] Society, the Ashmolean collections, and the volumes of the British Association. In the other order of his writings we have to enumerate—besides those for the elucidation of pure religion—the 'Connexion of Natural and Divine Truth,' 1838; the 'Unity of Worlds,' 1855; 'Christianity without Judaism,' 1857; on the 'Study of the Evidences of Christianity,' 1860, &c.

"During the last years of his life Mr. Powell was a controversial theologian. His writings were of a cast which is called liberal by all, but by some in one sense, by some in another. The freedom of his criticism could not but provoke strong attack from his opponents. Of the controversy this Society can take no cognisance, but it must be remarked, as a mere matter of biography, that in his assault upon some modes of theological thought, to which the University once appeared unalterably given, Mr. Powell seems to have been as much of a precursor as in his efforts to stimulate science. The last of his writings appears as one of seven 'Essays and Reviews,' most of his colleagues being Oxford men; and the whole of this work, which is in its fourth edition, shows that Oxford names of no mean note are now pledged to admit that freedom of examination which brought so much assault upon the isolated individual who first used it while actually connected with the University. Every meeting of educated men will contain those of the most opposite views as to his conclusions; but all will admire the fearless manner in which, without reference to his own interests, he spoke out the conclusions of his mind.

"In the year 1850 Baden Powell was appointed one of the Government Committee of inquiry into the studies enjoined in Oxford; he being well known as an ardent educational reformer, a fact pretty well evinced in his tract on 'State Education,' considered with reference to 'prevalent misconceptions on religious grounds.' In 1854 he was selected, at Aberdeen, one of the three judges to award the valuable Theological Burnett Prizes in that city. Nor was the range

of his capacity confined to argumentative studies only ; for he not only possessed a large fund of general information, but was also a proficient in painting, and was well practised in the choral harmonies of church music, in which science he was a strong amateur.

"The Professor's lamented death, on the 11th of June, 1860, was the result of an attack on the lungs which commenced in the previous winter. He had been duly warned of his danger by his medical friends and attendants, several weeks before he expired ; but he was resigned to the last, his only uneasiness being a difficulty of breathing, which, after a few days of extreme debility, terminated his existence. He died at his house in Stanhope Street, Hyde Park Gardens, at the age of 63, having scarcely ever been ill before."

Professor Powell was elected into the Royal Society in 1824, and repeatedly served on the Council. He was three times married, and leaves a numerous family. His widow is the daughter of Admiral W. H. Smyth, Fellow and formerly Foreign Secretary of the Royal Society.

**WILLIAM SIMMS** was born at Birmingham, in December 1793. His parents having removed to London, he was apprenticed to Mr. Bennet, a mathematical instrument maker, who had formerly worked under the eye of Ramsden. Mr. Simms afterwards commenced business at Bowman's Buildings, Aldersgate Street, where he constructed the large theodolites required by Colonel Colby for the Ordnance Survey, and established his reputation for excellence of workmanship. In 1826 he joined Troughton, and ultimately became sole proprietor of the well-known business in Fleet Street. The Cambridge Mural Circle, the whole of the instrument maker's part of the Greenwich Altazimuth, Transit Circle (for which he ground the 8-inch object-glass), Reflex Zenith-tube and Great Equatoreal, are monuments of his skill and ability. By the invention of his self-acting dividing machine, he reduced the process of graduation of instruments from a work of weeks to a work of hours, and with greater accuracy than before. The best evidence of the value of this machine appears in the fact that it was used to graduate the Zenith-distance Circle at the Greenwich Observatory. He took part also in the preliminary experiments required for the construction of the new Imperial Standard yard, and in the completion of that instrument.



Mr. Simms was a man of quiet unobtrusive manners, and of a fair and candid disposition, which won the esteem and, in not a few instances, the friendship of those with whom he transacted business. He became a Fellow of the Royal Astronomical Society in 1831, and served several times on the Council. He was elected a Fellow of the Royal Society in 1852. He died at his residence, Carshalton, June 21, 1860.

WILLIAM SPENCE was born of a respectable Yorkshire family at Hull in 1783: he commenced business in that town, and afterwards represented it in Parliament. At about ten years of age he was led, as he mentions in one of his addresses to the Entomological Society, "from mere boyish imitation to collect and dry plants, and to copy out the names of the Linnean classes and orders." This pursuit was interrupted by removal to a school, but he resumed it later, and when he had exhausted the plants of his neighbourhood, began the study of entomology with Coleoptera. In 1805, the present of a few insects, sent by the hand of a friend who was journeying to Suffolk, brought him into acquaintance, and eventually a lasting friendship, with the Rev. W. Kirby of Barham. To this friendship English literature owes a work which, with White's 'Selborne,' may be said to have done more to promote a taste for the study of natural history than any other of its class, and the first notion of this work was suggested by Mr. Spence in a letter to Mr. Kirby in 1808; but the authorities to be consulted were numerous, and the labour was great, so that the first volume did not appear until 1815. The second volume was published in 1817, the first having in the meantime passed through three editions. Mr. Spence now became subject to severe headaches and other signs of illness, which compelled him to abandon his studies for a while; he therefore, persuaded by his medical advisers, shut up his books, and quitted Yorkshire for Devonshire. During his residence at Exmouth he occupied himself with outdoor observations of insects; and his health having improved, the third and fourth volumes of the 'Introduction to Entomology, or Elements of the Natural History of Insects,' were brought out in 1826. Of this remarkable work it may be said without exaggeration, that it has charmed thousands of readers, and induced many to interest themselves in the study of insect life, who without it might

never have bestowed a thought on natural history. The complete work, including fifty-one letters, of which the whole of nine and parts of twenty-two others are from the pen of Mr. Spence, went through six editions prior to the decease of Mr. Kirby. In 1856 a seventh edition was published by the surviving author, more compact in form and lower in price than any which had preceded.

Seeking further to improve his health, Mr. Spence journeyed with his family to the continent in 1826, and remained abroad for eight years, during which he visited many of the principal cities of Europe, and passed the summer months in Switzerland. After returning to England he took up his residence in London, and employed his leisure in the promotion of science, particularly in the formation of the Entomological Society in 1833. Of that Society he remained, since Mr. Kirby's death, the sole honorary English member,—a distinction now confined exclusively to foreigners. In 1847, and on other occasions, he was elected President of the Society, and raised it by his exertions while in the chair, from weakness and depression, to strength of numbers and activity. He frequently attended the Society's meetings, as well as those of the Royal and Linnean Society, until increasing deafness deprived him of the pleasure of intercourse; but though thus withheld from his fellows by infirmity, he was always accessible when his advice was wanted, and ready to take trouble where encouragement could be given or assistance bestowed.

Mr. Spence was elected a Fellow of the Linnean Society in 1806, and of the Royal Society in 1834, and was chosen into their respective councils. He was author of numerous papers published in the Transactions of the Linnean and Entomological Society.

During his parliamentary career he laboured to promote measures for rendering Great Britain entirely independent of foreign commerce, and published a pamphlet on the subject, which at the time attracted much attention. He died on the 6th of January, 1860, at the age of 77.

**THEOPHILUS THOMPSON, M.D.**, was the son of Nathaniel Thompson, Esq., and was born at Islington on the 20th of September, 1807. His professional studies were commenced at St. Bartholomew's Hospital, prosecuted at Dublin and Paris, and completed at Edinburgh, where he took his degree in 1830. He settled in practice in London,

and was soon after elected Physician to the Northern Dispensary, and for fourteen years zealously discharged the duties of that office. In 1847 he was appointed Physician to the Hospital for Consumption, and much of his subsequent labour was specially directed to the investigation and treatment of that disease.

Dr. Thompson's chief writings are the following:—‘Annals of Influenza,’ published by the Sydenham Society, 1851; ‘Lectures on Diseases of the Chest,’ 1854, and ‘Lettsomian Lectures on Consumption,’ 1855; besides the chapters on “Hysteria,” “Neuralgia” and “Chorea” in the ‘Library of Medicine,’ edited by Dr. Tweedie, 1840. He also contributed various papers on professional subjects to the Medico-Chirurgical Transactions, and to different medical journals. He was elected a Fellow of the Royal Society in 1846, and is the author of two papers on the changes produced in the blood by the administration of oils, printed in the ‘Proceedings’ in 1854 and 1858.

Dr. Thompson was held in high esteem by his professional brethren, and was greatly beloved by his patients. He was gentle and modest in nature, but of inflexible moral integrity; and throughout his useful life he was actuated by a disinterested desire for the advancement of medical knowledge and the promotion of the right interests of his profession. He was a man of unaffected piety, which influenced him through life, and sustained him in resigned and cheerful submission under the afflicting malady which preceded his death. This consisted in a species of atrophy, which invaded his frame, and, without affecting his mind, eventually deprived him of all muscular power. He died on the 11th of August, 1860, from the immediate effect of bronchitis, which speedily prevailed over his well-nigh exhausted strength.

Dr. ROBERT BENTLEY TODD was born at Dublin on the 9th of April, 1809. His father, Charles Hawkes Todd, Professor of Anatomy and Surgery in the Royal College of Surgeons of Ireland, after attaining the greatest eminence and success in his profession, was cut off at the early age of 46, leaving sixteen children. Of this large family Dr. Todd was the second son; he was educated at Trinity College, Dublin, where he graduated in Arts and in Medicine, and afterwards passed a year in Paris in prosecution of his medical

studies. He then commenced practice as a Physician in London, and having obtained the Degree of M.D. from the University of Oxford, was in 1837 admitted a Fellow of the College of Physicians. About the time of his settling in London Dr. Todd became Teacher of Anatomy in the Aldersgate School of Medicine; and in 1836 he was appointed Professor of Physiology and of General and Morbid Anatomy in King's College. This Professorship he held until 1853, when he felt himself compelled to withdraw from it, in consequence of his increasing occupation as a Physician in large practice; meanwhile the opening of King's College Hospital, in the establishment of which he had taken an active part, afforded him a field for acquiring experience as a hospital physician, and for exercising his talent in clinical instruction, a duty he continued to perform until within a few weeks of his death.

Dr. Todd's was a life of active thought and steadfast work; and accordingly he soon acquired distinction as a medical teacher and writer, and as a scientific inquirer. About the outset of his career in London he projected the well-known 'Cyclopædia of Anatomy and Physiology,' of which he was the Editor, and to which he contributed several important articles. Some years later he published, in association with Mr. Bowman, 'The Physiological Anatomy and Physiology of Man,' a work abounding in valuable original matter. Of various subjects in Physiology which engaged his attention, the nervous system was perhaps that which interested him the most, and he has ably and lucidly written on its structure, functions, and diseases. But it was as a practical physician and clinical professor that Dr. Todd attained his greatest eminence. Some years before his death he had risen to the foremost rank in medical practice, and his clinical visits and lectures were assiduously attended by many devoted pupils. A selection of these lectures was published in three successive volumes. The last of these, completed only immediately before his death, contains an exposition of his views on the nature and treatment of acute inflammatory diseases,—a subject on which medical opinion has, from time to time, fluctuated between two extremes. Considering the zeal with which he inculcated his views and the boldness with which he exemplified them in actual practice, as well as his eminent and influential position, Dr. Todd may be said to have taken the lead among those who

in our time have advocated the stimulating and supporting, as opposed to the antiphlogistic and depleting mode of treatment. The present is not a fitting occasion for criticising opinion on questions in practical medicine; but this much we may be permitted to say, that even should the views maintained by Dr. Todd not ultimately prevail, a balance of good can scarcely fail to accrue from his rational exposition and discussion of the doctrine in his lectures and writings, and especially from his fearlessly submitting the results of its practical application to open scrutiny in a clinical hospital.

Dr. Todd was elected a Fellow of the Royal Society in 1838, and in 1838 and 1839 he served on the Council.

For some years before his death it was evident to Dr. Todd's medical friends that he was labouring under some serious internal disorder, but he continued actively to discharge his professional duties; at length, however, the crisis arrived, and he was cut off on the 30th of January, 1860, by repeated attacks of hæmatemesia, which was afterwards ascertained to have been caused by disease of the liver. He has left a widow and four children.

Besides the works of which the titles have been already mentioned, and single clinical lectures reported from time to time in the Medical Journals, Dr. Todd is the author of the following:—

'Clinical Lectures on Paralysis, Disease of the Brain and other affections of the Nervous System,' 1854.

'Clinical Lectures on certain Diseases of the Urinary Organs, and on Dropsies,' 1857.

'Clinical Lectures on certain Acute Diseases,' 1860.

'On Gout, Rheumatic Fever, and Chronic Rheumatism of the Joints,' 1843.

"On the Contractility or Irritability of the Muscles of Paralysed Limbs," &c., *Medico-Chirurgical Transactions* for 1847, vol. xxx.

"Additional Experiments on the Excitability of Paralysed and Healthy Limbs by the Galvanic Current," *Ibid.* 1853, vol. xxxvi.

Twenty-eight articles on Anatomy, Physiology and Morbid Anatomy, in the *Cyclopædia*. Those on the Nervous System were republished under the following title:—

"The Anatomy of the Brain, Spinal Cord, and Ganglions," 1845.

**HORACE HAYMAN WILSON** was born in 1786, and died in London on the 8th of May, 1860, at the age of 74, leaving a reputation of Eastern scholarship which has no equal among Englishmen, except in Sir William Jones and Henry Thomas Colebrooke. After receiving a good classical and professional education, he proceeded to India in 1808, in the Medical Service of the Government. He never, however, engaged in the practice of his profession, but being a skilful chemist and metallurgist, he was at once attached to the Mint of Calcutta, which stood in need of such services as he could render, and in due time he became Master of the Mint, a post which he held during his whole stay in India.

At Calcutta Mr. Wilson became acquainted with Colebrooke, and by his example and advice was induced to engage in the study of the Sanskrit language and literature. The fruits of his devotion were seen as early as 1813, when he published his first work, the translation in verse of a Sanskrit poem, called 'The Cloud Messenger.' This performance, equally distinguished by fidelity and good taste, at once established the author's reputation as an oriental scholar. From this time, the list of Mr. Wilson's various publications on oriental questions is too manifold for citation, and we need only name a few of the most remarkable. In 1819 he published the first, and in 1832 the second edition of his Sanskrit and English Dictionary, the first work of the kind that had ever been given to the public, the work which has been the main guide ever since of the student of the Sanskrit language and of comparative philology. This was followed by a lucid grammar of the complex and difficult Sanskrit, and by translations of the 'Vedas' and 'Puranas,' the most sacred writings of the Hindus. In 1840 he published his continuation of Mill's 'History of India,' in three volumes, and as late as 1854, his 'Glossary of Indian Terms,' a quarto of above 700 pages, a work replete with useful knowledge and ingenious criticism, in which Mr. Wilson shows his knowledge not only of the recondite language of India, but of its many vernacular idioms, as well as of the abundant Arabic and Persian words which have found their way into them.

While he was still absent in India in 1833, the University of Oxford chose him for the Boden Professorship of Sanskrit; and about the same time the Home Government of India named him Librarian and Curator of their Library and Collection of Natural History and

**Antiquities.** These lucrative appointments served only as incitements to further exertions, and Mr. Wilson's useful and valuable labours were continued almost to the day of his death.

Mr. Wilson was not a laborious scholar and a prolific writer only, he was also a careful and judicious inquirer. He was, indeed, a highly accomplished man; for with his profound knowledge of the languages and literature of the East, he combined a familiarity with the ancient and modern ones of Europe. To these he added skill in music and in the histrionic art, in which he was an excellent performer.

Mr. Wilson was elected a Fellow of the Royal Society in 1834. He married a grand-daughter of the celebrated Mrs. Siddons, and by her, who survives him, had a family of sons and daughters.

**M. LOUIS POINSOT** was born in Paris on the 3rd of January, 1777; quitted the École Polytechnique in the capacity of Ingénieur des Ponts et Chaussées in 1796; was appointed Professor at the Lycée Bonaparte; Professor and Examiner of the pupils who had completed the course of instruction at the École Polytechnique, and member of the Council for improving that institution. He was elected Member of the Institute in 1813, in the place rendered vacant by the death of Lagrange, by whom his future eminence had been predicted. He became Grand Officer of the Legion of Honour, Peer of France and Senator. He died, unmarried, on the 5th of December, 1859, at the age of nearly 83 years. The greater part of his life passed happily; while still young his talents were appreciated by the most eminent judges; in his advanced years he received the highest rewards from his countrymen. With his simple habits, the emoluments of the numerous and honourable appointments he held, and the profits of nine large editions of his 'Statics,' in addition to a moderate family inheritance, enabled him to leave at his death a fortune of upwards of 1,200,000 francs.

He was regarded as one of the most profound geometers in Europe. His philosophic mind suggested new methods of scientific investigation. His writings, always remarkable for their clearness and elegance, are frequently expressed in language divested of the technicalities of algebra. M. Chasles, alluding to Poinso't's new theory of rotation in the preface to his 'Géométrie supérieure,' expresses himself nearly in the following terms:—Nothing can be more beautiful than these direct,

lucid and graphic considerations, by means of which a great geometer, employing the ingenious doctrine of couples in dynamics, has unfolded all the geometric and dynamic properties of a heavy body in motion. . . . With the sole aid of geometric reasoning, M. Poinso<sup>t</sup> renders palpable, and depicts all the circumstances of the motion of a solid.

The titles and dates of M. Poinso<sup>t</sup>'s principal works are as follows:—"Sur les polygones et les polyèdres," *Journal de l'École Polytechnique*, 1809; "Mémoire sur l'application de l'Algèbre à la théorie des nombres et à la recherche des racines primitives," *Journal de l'École Polytechnique*, 1820; 'Eléments de Statique, suivis de quatre Mémoires'; "Sur la composition des moments et des aires dans la mécanique" (from the *Journal de l'École Polytechnique*, 1804); 'Sur le plan invariable du système du Monde'; 'Théorie générale de l'équilibre et du mouvement des systèmes'; 'Théorie nouvelle de la rotation des corda,' a classical work, in which M. Poinso<sup>t</sup> first propounded his beautiful theory of couples, and of which the 9th edition appeared in 1848; 'Recherches sur l'Analyse des sections angulaires,' 1825; 'Reflexions sur les principes fondamentaux de la théorie des nombres,' 1845; 'Théorie nouvelle de la rotation des corps,' 1851; 'Théorie des cônes circulaires roulants,' 1853; 'Sur la percussion des corps,' 1857; 'Précession des équinoxes,' 1857; 'Note sur la théorie des polyèdres,' 1858.

HEINRICH RATHKE was the son of a ship-carpenter of Dantzic, and was born in that town on the 25th of August, 1793. Being destined for the medical profession, he studied with that view in Göttingen and in Berlin, and in 1818 he commenced practice in his native city. While at Göttingen, however, he had acquired under the mastership of Blumenbach a decided taste for zootomical and zoological pursuits, and had, even in his student years, undertaken original researches in these branches of science. Accordingly, amid the more pressing occupations of professional life he continued to pursue with eagerness his favourite studies, and soon acquired the reputation of an able and accomplished biologist. His appointment to the chair of Physiology in Dorpat, to which he was called in 1828, enabled him thenceforward to give himself up entirely to scientific work; and, by the death of Eschscholtz, the sphere of his profes-



sorial duties was soon extended to Comparative Anatomy and Zoology. After seven years' agreeable and successful labour in Dorpat, Rathke was recalled to his native country to succeed his distinguished friend Von Baer, whom we still happily number among our Foreign Members, as Professor of Anatomy and Zoology in Königsberg, where he continued till the time of his death.

The chosen task of Rathke's scientific life was the investigation of the laws of formation of the animal body; and he soon perceived that the solution of the great questions involved was to be sought for not only in the examination of the mature structure, but also, and indeed with the best promise of success, in the study of the body in its embryo state, and in tracing its several parts through their successive phases of development up to their final condition. Zootomy and the study of embryonic development were in his hands mutually elucidative in determining the morphological equivalence of apparently dissimilar forms; and, in this way, passing from the one course of investigation to the other as the object of inquiry demanded, and labouring unremittingly as well as successfully, Rathke has earned a foremost place among those who have advanced the science of Animal Morphology.

Rathke's earliest researches were directed to the anatomy and development of Amphibia and Fishes. The most notable results were his various papers on the internal structure, in many ways so remarkable, of the Cyclostomes, and his monography on the development of the *Blennius viviparus*, the first thorough history of the embryogeny of a fish that had then appeared.

He next applied himself to the difficult problem of tracing the development of the genital system in the series of vertebrated animals, following out the metamorphoses of the Wolffian body in reptiles, birds, and mammals, and investigating its relation to the sexual organs. With the results of these inquiries appeared the account of his researches on the development of the respiratory apparatus, and the announcement of his well-known and important discovery that the embryos of all vertebrata have at a certain period the rudiments of a branchial system in form of arches and apertures, although without formation of real gills in the higher classes.

These extensive labours having been accomplished, and Von Baer's celebrated work on animal development having in the mean time

appeared, there remained, of the vertebrata, only the three orders of reptiles whose embryological history was stilled untouched. To these, therefore, Rathke resolved to extend his inquiries, and in 1838 produced his account of the embryogeny of the snake (*Coluber natrix*), and ten years later that of the Testudines. Both works are replete with new matter, and in that on the serpent especially the development of the several organs from commencement to maturity is handled with a depth and thoroughness of treatment hitherto unsurpassed. The third monography on the development of Crocodiles has been left by him nearly complete, and, we learn, will be published as a posthumous work.

The foundation of our more accurate knowledge of the embryology of the Articulata may be said to have been laid by Rathke's well-known memoir on the development of the river-crab, which appeared in 1829, and for which he obtained the Gold Medal of the French Academy of Sciences. He afterwards availed himself of the opportunities afforded him by his residence at Dorpat to extend his researches on the Crustacea; and with the view of prosecuting his inquiries on marine Invertebrata, he made a voyage from Dorpat to the Crimea in 1833, and another from Königsberg to the shores of Norway in 1839.

In the latter years of his life Rathke was engaged in two works which he has left nearly in a complete state—one on the development of the snail, the other on *Nephelis* and some allied annelides. He had long intended to publish a systematic treatise on Embryology and Comparative Anatomy founded on his academical lectures; this intention he did not live to carry out, but since his death the outlines of his lectures on the development of the vertebrata have been published under the title of "*Entwicklungsgeschichte der Wirbelthiere*," 8vo. Leipsic, 1861.

Indefatigably employed as an original inquirer, Rathke was none the less attentive to his duties as an Academical Professor. Up to the year of his death he was favoured with excellent health, but after suffering some time from severe catarrh, from which he partially recovered, he was cut off suddenly by an apoplectic seizure on the 15th of September, 1860.

Most of the eminent Societies of Europe enrolled Rathke among

their members; his election among the Foreign Members of the Royal Society took place in 1855\*.

\* The present brief sketch of the life and labours of Rathke has been taken principally from an obituary notice by Professor Zaddach in the Proceedings of the Physical and Economical Society of Königsberg for December 1860, which has been republished, together with a complete list of Rathke's writings, in the December Number of the N. Preuss. Provinc. Blätter, 3<sup>te</sup> Folge, vol. vi.

PROCEEDINGS  
OF  
THE ROYAL SOCIETY.

*November 30, 1860.*

ANNIVERSARY MEETING.

Major-General SABINE, R.A., Treasurer and Vice-President,  
in the Chair.

Dr. Tyndall reported, on the part of the Auditors of the Treasurer's Accounts, that the total receipts during the past year, including a balance of £420 carried from the preceding year, amounted to £4745 7s. 8d.; and the total expenditure during the same period amounted to £4247 15s. 8d., leaving a balance in the hands of the Treasurer of £497 12s.

The thanks of the Society were voted to the Treasurer and Auditors.

The Secretary read the following lists:—

Fellows deceased.

Sir Charles Barry, R.A.	Lieut.-Colonel Leake.
General Sir Thomas Brisbane.	Joseph Locke, Esq.
Robert Edward Broughton, Esq.	The Right Hon. Lord Londesborough.
George Buist, Esq., D.C.L.	The Right Hon. Lord Macaulay.
John Frederick, Earl Cawdor.	Charles May, Esq.
Henry Drummond, Esq.	Joseph Miller, Esq.
Sir Fortunatus Dwarries.	Major Moore.
Sir William Browne Ffolkes, Bt.	M. César Moreau.
Lieut.-Gen. Sir Robert Harvey.	John Narrien, Esq.
Thomas Hoblyn, Esq.	Charles H. Parry, M.D.
Rev. David Laing.	

Rev. Baden Powell.  
 Charles Gordon, Duke of Richmond.  
 William Simms, Esq.  
 Lieut.-Colonel Charles Smith.  
 William Somerville, M.D.  
 William Spence, Esq.

Sir John Edward Swinburne, Bart.  
 Theophilus Thomson, M.D.  
 Robert Bentley Todd, M.D.  
 Rev. John Traherne.  
 John Ashley Warre, Esq.  
 Major-General Wavell.  
 Horace Hayman Wilson, Esq.

*On the Foreign List.*

Louis Poinso.  
 Heinrich Rathke.

*Withdrawn.*

George T. Doo, Esq.  
 Samuel Peace Pratt, Esq.

*Defaulter.*

Robert William Sievier, Esq.

List of Fellows elected since the last Anniversary.

Frederick Augustus Abel, Esq.  
 Thomas Baring, Esq., M.P.  
 John Frederick Bateman, Esq.  
 Right Hon. Edward, Lord Belper.  
 Edward Brown-Séquard, M.D.  
 Richard Christopher Carrington, Esq.  
 Right Hon. Frederick, Earl De Grey and Ripon.  
 Right Hon. Sir William Erle.  
 Francis Galton, Esq.  
 Joseph Henry Gilbert, Esq.  
 Sir William Jardine, Bart.

Thomas Hewitt Key, Esq., M.A.  
 Joseph Lister, Esq.  
 Rev. Robert Main, M.A.  
 Robert William Mylne, Esq.  
 Roundell Palmer, Esq., Q.C.  
 Right Hon. Sir Edward Ryan.  
 John Thomas Quekett, Esq.  
 Right Hon. George, Earl of Sheffield.  
 Edward Smith, M.D.  
 Rt. Hon. Edward, Lord Stanley.  
 Right Hon. Spencer Horatio Walpole.

*On the Foreign List.*

Alexander Dallas Bache.  
 Hermann Helmholtz.  
 Albert Kölliker.  
 Philippe Edouard Poullétier de Verneuil.

The Chairman then addressed the Society as follows :—

GENTLEMEN,

It is known to you all that early in the summer our excellent President suffered from an affection of the eyes, which was partially relieved by an operation, and that he is now about to undergo a second operation for the removal of a cataract. Under these circumstances, Sir Benjamin Brodie, prompted by his well-known unwillingness to hold an office when temporarily disqualified for the fullest performance of its duties, signified, by letter to the Council towards the end of September, his desire to decline to be put in nomination for the Presidency at the ensuing (that is the present) election. The Council having duly considered Sir B. Brodie's letter, and looking forward with confident hope and expectation to the recovery of his sight by the impending operation, were unanimously of opinion that the interests of the Royal Society would be most effectually promoted by his allowing himself to be again put in nomination for the Presidency ; feeling assured also that his consenting to do so would be in accordance with the general wish of the Society. The Council further requested me, as the Treasurer of the Society, and in the absence of the President officiating as Chairman of the Council, to communicate personally with Sir Benjamin Brodie (then at Tunbridge Wells), and to convey the earnest hope of the Council that he would comply with their wishes. Thus solicited, Sir B. Brodie has consented to be again placed in nomination for the office of your President. In making this communication, it is fitting that I should add that, in the opinion of his medical advisers, the probability of the operation which he is about to undergo being successful in its issue is very strong, and that they have a confident hope that his sight will be sufficiently restored to enable him to reassume the duties in the spring, if it should now be your pleasure to re-elect him.

Sir B. Brodie has placed in my hands an Address, which, had he been able to have taken the Chair at this Anniversary Meeting, it was his intention to have delivered himself. This address he has requested me to read to you, which I shall proceed to do after I shall have briefly noticed some subjects which have occupied the attention of your Council in the past year, and on which it is desirable that the Society should be informed.

1. *Scientific Relief Fund.*—The intention of several Fellows of the Society to promote the establishment of a Permanent Fund, to be invested in the name of the Royal Society, the interest of which Fund should be applied (under the control of the Council) to the aid of such scientific men and their families as may from time to time require and deserve assistance, having been notified to the Council, accompanied by a list of several intended donations, the Council passed a resolution accepting the proposed Trust, and directing that all Donations on account thereof, paid to the Bankers of the Society, should be invested in Government Securities, and constitute a Fund to be named the “Scientific Relief Fund,” the interest of which should be at the disposal of the President and Council under the following conditions, viz. :—

“That the President and Council of the Royal Society have full power to appoint Committees, or to make such arrangements as they may, from time to time, consider most advantageous for the carrying out the objects of the Trust in the most liberal sense.

“That no application for relief be entertained except on the recommendation of the President of one of the following Chartered Societies :—The Astronomical, Chemical, Geographical, Geological, Linnean, or Royal Society ; it being understood that the several Presidents will consult their respective Councils as to the persons whom they intend to recommend for relief.

“That in each case in which assistance is granted, a record of the recommendation be entered on the Minutes of the Council of the Royal Society.

“That the amounts and particulars of the Fund, as invested, with the account of the receipts during the preceding year, the gross expenditure and disposable balance in hand, appear in the annual published Financial Statement of the Royal Society.”

The Council have made arrangements for the administration of the proceeds of this Trust-Fund in conformity with the above conditions. The sum invested in the New 3 per Cent. Annuities amounts at present to £4824 9s. 9d.

2. *Increased Gratuities to the Secretaries.*—The duties of the Secretaries having augmented greatly of late years, in consequence partly of the increased activity with which various branches of science are cultivated in this country and the greater number of papers

annually presented to the Society, and partly from new duties devolving on them, it appeared to the Council desirable to reconsider the amounts of the "honoraria" presented annually to those gentlemen as an acknowledgement, but by no means as a remuneration, of their services. As a preliminary step, a Committee \* was appointed to inquire into and to report on the duties of the Secretaries at various times, and on the gratuities which have been awarded them, as well as on the present financial condition of the Society. With your permission I will read the Report of this Committee.

"Your Committee beg to report to the Council, that, in performing the task which was imposed upon them, they have inquired into the duties of the Secretaries at various times, the gratuities which have been awarded to them, and the financial condition of the Society.

"They have been favoured with valuable information and opinions by former Officers of the Royal Society,—Sir John Herschel, Mr. Brande, Dr. Roget, and Sir John Lubbock.

"Previous to the year 1720 no regular Salaries were assigned to the Secretaries, but it was customary to present them from time to time with sums varying from £10 to £20 under the name of 'Gratuities.'

"In 1720, on the motion of the President, Sir Isaac Newton, the Council directed that £50 should be paid to each of the two Secretaries annually. In 1732 this amount was increased to £60, and in 1760 to £70 10s. In November 1799, on the motion of the President, Sir Joseph Banks, the amount of the Salaries was reconsidered by the Council, and raised to *One Hundred Guineas* to each Secretary, at which amount they have remained from that time to the present.

"The office of 'Foreign Secretary' originated in a legacy of £500 bequeathed to the Society in 1719 by Mr. Robert Keck, for the express purpose of remunerating a person for carrying on foreign correspondence. In 1720 the first Foreign Secretary was appointed, with a Salary of £20 a year, which sum has been paid, without increase, from that time to the present.

"It is the concurrent opinion of all who have the best means of knowing, that since the gratuities were last fixed in 1799 the busi-

\* Consisting of the President, the Treasurer, the Rev. J. Barlow, Mr. Bell, and Dr. W. Farr.



ness of the Society and the duties of the Secretaries have largely increased. The increase of Fellows and the larger income of the Society have enabled it to extend its operations. In the ten years 1790 to 1799, 319 papers were communicated to the Society; and in the ten years 1850 to 1859, the number of such papers was 672. Some of the communications are short notices for publication in the 'Proceedings,' and it is impossible to determine precisely in what ratio the work has increased; but your Committee are disposed to believe that it is represented approximately by the above figures. The Secretaries now edit the 'Transactions' and the 'Proceedings,' which are found so useful by the Fellows, and this latter duty has added considerably to their labour.

"The current revenue of the Society may be set down at about £3514, of which £1150 are derived from rents and dividends, and £517 from the Stevenson bequest. The latter sum, it is known, will increase as lives fall in. The annual subscriptions amount to £1126; the entry fees, estimated on an average of eleven years, will be about £170; the compositions £360; the 'Transactions' will yield £276; making the aggregate revenue under these heads £1932. Your Committee see no reason to believe that these sources of income are likely to fail.

"The current annual expenditure may be stated at about £2839; namely, £1177 on printing; £764 on gratuities, salaries and wages; £187 on books and binding; £511 on house expenses; £200 on Catalogue of Periodicals. These items necessarily fluctuate, and the printing bill last year exceeded considerably the above amount; but the amount just stated for printing is estimated from an average of the last eleven years. The income of the Society has thus for some years exceeded the expenditure by about £675.

"Looking at the duties which now devolve upon the Secretaries, of regularly attending Meetings, reading papers, editing the 'Transactions,' preparing the 'Proceedings' for publication, and other work,—looking also at the remuneration which it is found desirable to give gentlemen who discharge less onerous duties merely as editors of literary works in the present day,—your Committee are of opinion that the Council will be acting quite in conformity with the sound principles which were laid down in Sir Isaac Newton's presidency, and have been acted on since, by increasing the gratuity to each of

the two Secretaries. As the result of the inquiries made by your Committee, they would suggest that the addition should be £95, raising each gratuity from £105 to £200. This would involve an increase of £190 in the expenditure.

"The relations of the Society with foreign countries may be largely extended, and your Committee are of opinion that to accomplish this object £80 may be advantageously added to the £20 now voted, making the annual gratuity of the Foreign Secretary £100.

"The total augmentation of the expenditure under this arrangement would be £270, leaving a probable annual surplus of £400 to be devoted to the numerous purposes which fall naturally within the scope of the Society's inquiries.

"Your Committee are of opinion that the offices efficiently discharged will still be to a great extent honorary; and that so long as the Society is so fortunate as to have able, industrious, and eminent men as its Secretaries it will be still largely in their debt.

"Should the finances of the Society, through any unforeseen circumstance, require it, there would not, your Committee apprehend, be any difficulty in again revising the scale of gratuities which may be awarded."

The recommendations of the Committee regarding the augmentation of the gratuities have been adopted by the Council; and the view taken by the Committee of the financial condition of the Society has been thus far confirmed by an excess in the present year of income over expenditure (the increased gratuities being included in the latter) of £742 2s.

3. *Duties of the Foreign Secretary.*—The Council having thus augmented the honorarium of the Foreign Secretary, have had under their consideration the advantage which the Society might derive from an extension of the duties of the Foreign Secretary as hitherto defined by the Statutes, and have made additions to those duties under the three following heads, viz. :—

"1. To prepare the Biographical Notices of deceased Foreign Members for publication in the 'Proceedings.'

"2. To collect such information respecting the labours and discoveries of Foreign Men of Science as may serve to aid the Council in the nomination of persons for election by the Society as Foreign Members, and in the award of the Copley and Rumford Medals.

“ 3. To furnish the Society, from time to time, with early notice of researches of special importance carried on abroad ; such notice to be drawn up in the form of a short communication, to be read as early as practicable at an evening Meeting of the Society, and published in the ‘ Proceedings.’ ”

These additions are to be regarded for the present simply in the light of *Regulations made under the authority of the Council*. They may hereafter be embodied in the Statutes, if after trial it shall appear desirable to do so, with any further additions or modifications which experience may point out.

I shall now proceed, with your permission, to read Sir Benjamin Brodie’s Address.

“ Since our last Anniversary, the Royal Society has continued to pursue its mission, and I hope that I may add, with no unsuccessful result. Papers of great importance have been given to the world in the last volume of the ‘Philosophical Transactions;’ and many others which are of much interest may be found in the lately published Numbers of our ‘Proceedings.’ This last publication has become a valuable addition to scientific literature, and, as such, has risen in estimation both in this and in foreign countries, beyond the expectation of those by whom it was originally suggested. The meetings of the Society have been fully attended ; and the occasional dryness of scientific details has been not unfrequently relieved by the display of new experiments, and by discussions in which many of our Fellows have taken part, uninfluenced by any other desire than that of mutually giving and receiving information. The increasing number of candidates for admission into the Royal Society sufficiently shows how highly that honour is appreciated by the public ; and I may take this opportunity of repeating an observation which I made formerly, namely, that this distinction, like those afforded by the Universities, is all the more valuable to those on whom it is conferred, inasmuch as it is one of the very few which cannot be obtained either by the favour of the great or through the partiality of friends. As the election of the Fellows is now conducted, it is barely possible that that honour should be on any occasion improperly bestowed. There can indeed be no doubt that the present mode of election has been a great improvement on that which had been adopted previously, and

that it has very much contributed to maintain the honour and dignity of our Institution. I make this acknowledgement the more readily, because I must own that I was not a convert to the new system in the first instance. It was perhaps because I had been intimate with the Royal Society from a very early period of my life, that, when the change was first suggested, I was led to believe, in common with my friend Robert Brown and some other of the older Fellows, that it would have been better for us *stare super antiquas vias*. Experience has altered my opinion on the subject.

“ It would, however, be unworthy of us, as the living representatives of those great men by whom the Royal Society was founded, to consider the progress of the physical sciences only as it regards ourselves. Looking abroad into what is going on in general society, I am sure that there is no individual now present who is not gratified to find that there is a desire to become acquainted with natural phenomena and the laws which govern them, much beyond what existed even at the beginning of the present century ; and that the opportunity of satisfying that desire, to a certain extent, is afforded to persons of every class, not only in the metropolis, but also in the provincial towns, and sometimes even in our villages, by means of Mechanics’ and Literary Institutions, and by occasional lectures where no such Institutions exist. As a part of the education of those who ought to be the best instructed members of the community, in our schools and colleges the study of the physical sciences has already taken root ; and there is every reason to believe that the tree will grow and flourish.

“ In the address which I offered to you at our last Anniversary, I adverted to the influence which such studies must have in training some of the higher faculties of the mind ; and I also adverted to the effect which they have already produced in laying the foundation of a better method of investigation in other departments of knowledge. It is not my intention to trouble you with a repetition of these observations ; there are, however, some other points belonging to the same subject, to which I would willingly draw your attention.

“ In holding the opinion that much advantage would arise from the study of the physical sciences being regarded as an essential part of a liberal education, I apprehend that it has never entered into the mind of any person who has seriously reflected on the subject, that it should

supersede those other studies which form the basis of such an education in this and others of the more highly civilized communities at the present day. One of these studies, indeed, namely mathematics, is necessary to the physical sciences themselves, there being no one of these sciences to which, under certain circumstances, mathematical reasoning may not be usefully applied ; whilst one especially, and that the foremost and grandest and most important of the whole, is so entirely founded upon it, that, if this were taken away, there would be very little of the science left. The languages of the ancient nations of Greece and Italy have no such direct relations to the physical sciences as mathematics ; but I know no better method than that which the study of them affords of training the mind, at an early period of life, to habits of thought and attention, and so of fitting it for other studies afterwards. There is another advantage to be derived from those pursuits which lead us to a knowledge of the Greek and Roman classics. Greek and Roman literature has been the foundation of the best literature of modern Europe ; and an acquaintance with it stores the mind of the youthful student with graceful recollections and noble thoughts, which may exercise a wholesome influence over him through all the rest of his life. Further, it may be observed that the study of the ancient languages is an excellent introduction to a knowledge of grammar, and of the use of language generally, and this knowledge is of a kind the importance of which cannot well be over-estimated ; not only as it is by means of language that we are enabled to communicate our thoughts to each other and hand them down to those who are to come after us, but because language is in itself an instrument of thought, without which the reasoning powers which God has given us could be turned only to a very small account.

“ There is indeed no sufficient reason why the instruction of youth should be limited to one of these subjects, to the exclusion of the other, there being “ ample room, and verge enough ” for both ; it being quite true, as has been lately observed in an address delivered at Edinburgh by a noble Lord, a Fellow of our Society, that what is wanted in education is not so much that a great deal should be learned of any one subject, as that whatever is learned should be learned thoroughly ; so that the student should acquire the habit, so important in after life, of undertaking nothing which he does not

undertake in earnest. One object of education undoubtedly is to furnish the mind with knowledge which may be turned to good account hereafter. But that is not the only object. And there is always danger that, in exercising the faculty of learning over much, the higher faculties of thought and observation may not be exercised sufficiently. There may indeed well be, for the higher order of minds, too much as well as too little of systematic education; and hence it is that for some of the greatest achievements in the way of scientific discovery, we are indebted to those who, like Sir Humphry Davy, were in a great degree self-educated.

“It is a poor pedantry that would exalt one kind of knowledge by disparaging others. Literature, the arts, the moral and the physical sciences, all of these in their respective ways have tended to elevate the condition of mankind. But it is by the union of the whole that the greatest results have been obtained. That union is indeed as necessary to the higher forms of civilization, as the combination of rays of different degrees of refrangibility is to the constitution of a beam of solar light.

“Of the physical sciences, it may, I apprehend, be truly asserted that they have an advantage over every other department of knowledge,—in this respect, that the field of inquiry is practically unlimited. The student may indeed meet with an impassable barrier in one direction, but in that case he has only to proceed in another. As he advances, the horizon which terminates his view recedes before him. He enters on fresh scenes, gathers in new knowledge; and every addition which he makes becomes the foundation of further knowledge, to be afterwards acquired; so that, at the end of a long life, he finds himself a learner still. In the meanwhile, under whatever circumstances he may be placed,—whether he be in the cultivated valley, on the glaciers of the Alps, on the wide sea, in the crowded city, in the busy factory, in the broad sunshine, or in the starlight night,—he has only to look around him to find objects which have to him a peculiar interest, exhibiting relations which are not perceptible to those whose minds have been otherwise engaged. While viewing the gorgeous sunset, he finds, in the changing colour of the clouds and in the dark blue sky above, illustrations of the phenomena and laws of light. The flashes of the aurora are to him not mere objects of

curiosity, but are associated with the magnetism of the earth,—with that mysterious force which, like the force of gravity, connects us with the sun, and probably with all the other heavenly bodies, even those which are at the greatest distance from us. In the tumultuous movements of the atmosphere, which tear up trees by their roots, and cause the destruction of life by shipwreck, he recognizes the law of storms, and is enabled to comprehend how the mariner, by steering his course in one direction, may avoid those dangers to which he would be exposed if he were to steer it in another. In this way it is plain that even a moderate acquaintance with the physical sciences cannot fail to add to the interest of life ; an advantage which, under occasional circumstances, may be extended even to the humbler classes of society. A professor of one of our ancient universities, and a distinguished Fellow of the Linnæan Society, does not consider it to be incompatible with his duties as a parish priest, nor beneath his dignity as a philosopher, to give such simple instructions in Botany to the girls of the village in which he resides as may enable them to understand the flora of the neighbouring district ; thus affording them not only a useful, but a cheerful occupation for hours which would otherwise be passed in idleness.

“ It was on the 28th of November, just now 200 years ago, that several eminent individuals, who had previously been in the habit of meeting for the purpose of communicating with each other on subjects of common interest, assembled in Gresham College, and agreed to form themselves into a Society, having for its object the prosecuting of physico-mathematical experimental learning. When they reassembled on the following week, it was reported to them that what they proposed was highly approved by the reigning Monarch ; who intimated at the same time his desire to do what lay in his power towards promoting so useful an undertaking. Accordingly steps were taken for the incorporation of the Society under a Royal Charter, that Charter being conferred on them in due form two years afterwards. Such was the origin of the Institution which I have now the honour to address ; and to which the world is indebted for the long series of scientific memoirs contained in the 150 volumes of the ‘ Philosophical Transactions.’ The publication of these ‘ Transactions,’

however, was not begun until the year 1665, and then only in the form of a few pages, produced at uncertain intervals, which, being collected, made a thin volume at the end of the year.

“Many years elapsed before the ‘Philosophical Transactions’ became of larger dimensions. But we are not therefore to suppose, because so little was done in the way of publication, that little was really done for the promotion of the objects which the Founders of the Royal Society had in view. At this time Lord Bacon had already pointed out the right method to be pursued for the advancement of learning; and the abstract science of Geometry, inherited from an ancient nation, had been partially applied in the investigation of the physical sciences. Nevertheless it cannot be said that these sciences were more than in an infant state; and some which are now among the greatest subjects of attention, for instance Chemistry and Geology, had barely been called into existence. There was indeed as yet no sufficient number of facts collected on which the superstructure of science could be raised. The Founders of the Royal Society well comprehended what was required. If I may be allowed to use a homely expression, they had the good sense to begin at the beginning; and their first endeavours were to collect a larger number of facts by a course of experimental inquiry. Dr. Birch’s ‘History,’ which occupies a period of twenty-seven years from the foundation of the Royal Society, furnishes us with a great deal of valuable information as to this part of their labours, and gives us a more just notion of what the Royal Society accomplished in those days, than can be obtained from the ‘Philosophical Transactions’ themselves. At the several Meetings experiments were suggested, which were afterwards carried into effect. Mr., afterwards Dr. Hooke, received a special appointment as experimentalist; for which office he was well qualified, not less by his practical skill, than by his great and discursive genius. The collection of the experiments proposed and made during the period which I have mentioned would of itself form an instructive volume. It might not indeed add much to our present knowledge, but it would show us in what manner much of that knowledge with which we are now familiar had its origin, and at the same time furnish a grand example of the caution and circumspection with which all experimental inquiries should be conducted.

“With the gradual extension of knowledge, the method of inquiry



necessarily became modified. The size of the volumes of the 'Philosophical Transactions' gradually increased, and longer and more elaborate memoirs superseded the brief notices of which the earlier volumes were composed.

"It is not for us to give nor even to form an opinion of what the Royal Society has done during the last few years; but we are at liberty to refer to what has been done by our predecessors; and with regard to them we are justified in the conclusion that they have well performed the task which they had undertaken. In adding to human knowledge, they have added to human happiness. Standing apart from politics, they have pursued an independent course, having no selfish objects in view, but acting harmoniously with the Government of the day, whatever it might be. I am sure that every existing Fellow of the Society will join with me in the desire that we and those who come after us may continue in the same path, so as to maintain the dignity of science, and do honour to our country:—

*'Alterum in lustrum meliusque semper  
Proroget ævum.'*"

On the motion of Mr. Horner, seconded by Sir R. Murchison, it was resolved,—

"That the account of the proceedings of the Council communicated from the Chair, be printed and circulated among the Fellows.

"That the thanks of the Society be returned to the President for his Address, with the expression of sincere regret for the cause of his absence; and that he be requested to permit his Address to be printed."

The Chairman then announced the adjudication of the Medals, and presented them in the following terms.

The Copley Medal has been awarded to Professor Robert Wilhelm Bunsen, of Heidelberg, one of our Foreign Members, for his researches on Cacodyl, Gaseous Analysis, the Volcanic Phenomena of Iceland, and other researches.

For thirty years Professor Bunsen has been a most industrious and successful investigator of various subjects in the domain of chemistry. His researches on Cacodyl supplied the first instance of a compound radical, behaving through a complete series of reactions like an elementary body, and being prepared in the free state

by processes similar to those by which many metals are prepared from their compounds. Notwithstanding the extraordinary difficulties of the research, Professor Bunsen carried it through completely, and with such accuracy that every one of his results has stood the test of subsequent investigation, and not one of his formulæ has been altered. The difficulty and danger attending these manipulations with spontaneously inflammable, highly poisonous and volatile bodies, are quite unparalleled, and enhance our admiration of the singular perseverance with which the subject was followed out into its most minute ramifications. The importance of the results, and the value of the influence which they have exerted on the development of sound views in organic chemistry can scarcely be over-estimated. The most important investigations in subsequent years have been mainly applications of the theory of organic radicals to new bodies, or extensions of its use in cases in which it had been already applied.

Another most important service to science, was Professor Bunsen's invention of a simple and effective apparatus for analysing gases, with an accuracy previously unattained. His researches on this head may justly entitle him to be regarded as the founder of accurate gas-analysis,—a branch of quantitative analysis which now far excels all others in the perfection of its results.

His researches on the gases evolved from blast-furnaces consuming charcoal, coke and coal (the latter made in conjunction with Dr. Playfair), are models of experimental and analytical skill, and have led to important applications of the waste gases of such furnaces to useful purposes.

Professor Bunsen's researches on the volcanic phenomena of Iceland, besides being valuable contributions to mineral chemistry, exhibit a masterly application of chemistry and physics to geological phenomena.

It is proper to remark that the important researches in which Professor Bunsen is now engaged with Professor Kirchhoff, not being yet complete, do not form any part of the grounds on which the Medal has been awarded.

**PROFESSOR MILLER,**

In transmitting this Medal to Professor Bunsen, you will not fail

to convey to him the deep interest with which the Royal Society regards the progress of the very important researches, in which he is engaged in conjunction with Professor Kirchhoff.

A Royal Medal has been awarded to Mr. William Fairbairn, F.R.S., for his various Experimental Inquiries on the Properties of the Materials employed in Mechanical Construction, contained in the 'Philosophical Transactions' and in the publications of other Scientific Societies.

Mr. Fairbairn's first scientific publication was, I believe, a small volume published in 1831 on 'Canal Navigation,' illustrative of the advantage of the use of *Steam* as a moving power on canals. In this work he detailed the results of experiments made on the Forth and Clyde canal and on other canals in Scotland, and recorded experiments made about the same time by other parties as to the traction of light boats by horse-power at high velocities.

In 1837 he contributed his first paper to the Manchester Literary and Philosophical Society, being "An Experimental Inquiry into the Strength and other Properties of Cast Iron." This was the first of many valuable papers on the same and similar subjects which have been from time to time published in the 'Memoirs of the Manchester Society,' in those of the British Association and of the Institution of Civil Engineers, and in the 'Philosophical Transactions.' It is well known also, and has been warmly acknowledged by Mr. Eaton Hodgkinson, that Mr. Fairbairn rendered most valuable assistance to Mr. Hodgkinson in his experimental inquiries into the strength of iron (for which the Royal Medal was awarded in 1841), by placing at Mr. Hodgkinson's disposal his works at Manchester, in which the experiments were conducted free of cost.

In the Transactions of the Manchester Geological Society in 1841, there is a paper by Mr. Fairbairn on the economy of raising water from Coal Mines; and in Weale's Quarterly Papers on Engineering, an account of his "Ventilated Water-wheel," a very material improvement in the construction of water-wheels.

Since that time his experimental researches and his contributions to various scientific bodies have been very numerous and important. Amongst these I may specify Papers on Fire-proof Constructions—On the Strength of Iron and the best Form of Beams and Pillars—

On Boiler Explosions—On the Collapse of Tubes under Pressure—On the Density of Steam—On the principle of Construction of Iron Ships—and on other kindred subjects : many of these being within the period required by the terms of the award of the Royal Medal. Of separate works may be noticed a volume on the Conway and Britannia Tubular Bridges (1849), including his researches on the best form and strength of *wrought* iron beams. A work ‘On the Application of Cast and Wrought Iron to Building purposes’ (in 1854 and 1857), and a valuable compendium entitled “Useful Information for Engineers,” in 1856 and 1860.

Perhaps it may be said with truth that there is no single individual living who has done so much for practical science, who has made so many careful experimental inquiries on subjects of primary importance to the commercial and manufacturing interests of the country, or who has so liberally contributed them to the world.

MR. FAIRBAIRN,

In presenting this Medal to you from this Chair, I will venture to say that the award of the *Royal Medal*,—the Medal which Her Majesty the Queen has been graciously pleased to place at the disposal of the President and Council,—for scientific services such as yours, so eminently conducive as they have been to the *general* good, is even *peculiarly* appropriate.

A Royal Medal has been awarded to Dr. Augustus Waller, for his investigations into the Anatomy and Physiology of the Nervous System, and for the introduction of a valuable method of conducting such investigations.

Previously to Dr. Waller’s inquiries it was known that, when a nerve is cut across, the distal portion, severed from its central connexion, undergoes disorganization ; but Dr. Waller was the first to conceive, or at least to make known the idea of employing the section and disorganization of a nerve as a means of tracing out its distribution. In the art of injection anatomists have long possessed a method of following even the finest branches of the blood-vessels ; and in like manner the well-marked alteration in the aspect of a nerve consequent on its section and degeneration has been made available by Dr. Waller for identifying and following its ramifications and finest divi-

sions when mingled with other nerves distributed to the same organ, or for tracing its fibres with certainty through the intricacies of a plexus to which various nerves contribute. He has suggested, too, the employment of this method to determine the course and connexions of particular tracts of nervous fibres in the brain and spinal cord.

Dr. Waller has himself applied this method with much success in unravelling the ultimate distribution of the nerves of the tongue, and in investigating the constitution of the cervical part of the sympathetic nerve and its relation to the spinal cord ; and his process has also been successfully employed by others in similar inquiries.

But the application of Dr. Waller's process is not confined to anatomical investigation alone. He has shown how it may serve also to elucidate the functions of nerves. The disorganization of nerve-fibres consequent on their section involves the loss of their functional activity ; and, accordingly, when a nerve contains fibres derived from two or more sources and operates functionally on several different organs, its function may be analysed, as it were, by the separate section and disorganization of its different tributaries and consequent elimination of the special function of each from the general effect. In this way Dr. Waller has been able to show that the influence of the pneumogastric nerve over the motion of the heart, and in great part also its government of the motions of the larynx, depend on fibres which are contributed to that nerve by the spinal accessory.

Having once perceived that the changes consequent on the division of nerves might afford a valuable means of research, Dr. Waller was naturally led to study with care the progress and characteristic features of the disorganizing process, and the modifications of it depending on the nature of the animal subjected to experiment, on age, external temperature, and other circumstances. In the course of these investigations he found that, when the cut ends of a divided nerve are reunited by the healing process, the fibres of the distal or severed part first suffer atrophy throughout their whole length, and are then regenerated, or at least restored to their original integrity.

Another result of this inquiry was the discovery of an important relation between the ganglia of the spinal nerves and the nutrition of their sentient fibres. This fact, previously unknown, but now fully

established, may probably help to a further insight into the nature and office of nervous ganglia, a question in physiology on which our want of real knowledge has given scope to much profitless speculation; its discovery was deemed of sufficient importance by the French Academy of Sciences to entitle its author to the award of the Monthyon Prize in Physiology for the year 1856.

Besides these independent researches, Dr. Waller, in association with Professor Budge of Bonn, undertook an experimental inquiry into the influence of the sympathetic nerve on the motions of the iris, which resulted in showing the dependence of certain of these motions on a particular part of the spinal cord. For these researches the authors obtained the Monthyon Prize for 1852. Lastly, through his independent investigations, Dr. Waller had an important share in the discovery of the influence of the cervical part of the sympathetic nerve on the contractility of the blood-vessels of the head; and he first demonstrated, by positive experiment, the influence of the cilio-spinal region of the spinal cord over these vessels.

For these various merits the Council have adjudged to Dr. Waller a Royal Medal, but especially for having supplied a process of research, which, already successful in the hands of himself and other inquirers, promises to afford most effectual aid in the future study of the Nervous System.

#### DR. WALLER,

In presenting this Medal to you, I have to express the hope of the Council that it will operate as an encouragement to you to prosecute researches which are considered by your brother physiologists to have so high a value.

The Rumford Medal has been awarded to Professor James Clerk Maxwell, for his Researches on the Composition of Colours and other Optical Papers.

Professor Maxwell is the author of various remarkable papers on subjects of pure mathematics and physics, which cannot here be mentioned, besides his memoirs more immediately devoted to optics. In one of his earliest papers, he has connected, by rigorous calcula-

tion, the mechanical strains to which elastic solids are subjected under certain conditions with the coloured curves which those solids exhibit in polarized light. In a paper published in the 'Quarterly Journal of Mathematics,' he has treated in a very general manner the passage of rays of light through optical combinations.

The research specially mentioned in the award of the Rumford Medal, was commenced by him many years ago, and as early as 1852 he had made an instrument for examining the mixture of the colours of the spectrum; but the first paper which he published on the subject was read before the Royal Society of Edinburgh in 1855. Helmholtz had in the meantime published some important investigations on the composition of the colours of the spectrum; but as the main object of his research was merely to make out under what circumstances two colours of the spectrum could be combined so as to give white light, his results could not be applied to test quantitatively a mathematical theory of the composition of colours.

Such a theory is virtually contained in the empirical construction which Newton long ago gave for determining the effect of combining in any proportions the colours of the spectrum. Newton's construction appears, however, to have been based on estimation only; and even the mathematical theory involved in it does not seem to have been clearly pointed out; though it has long been known, or suspected, that colour depends in some way on three elements, whether the triplicity exist objectively in some quality of the light itself, or subjectively in our organization. In the paper already mentioned, Professor Maxwell has not only exhibited in its essential simplicity the theory involved in Newton's rule, but has put the theory to the test of exact quantitative experiments. The instrument which he devised for that purpose, and which he calls a 'colour-top,' enables the observer to establish an exact match between two different sets of coloured papers, the colours of which are mixed by rotation in proportions which can be read off by the graduation. Such a match can always be established between any four colours (including white as a colour), by properly varying their proportions. If we suppose ( $n$ ) observations to have been taken with the instrument, establishing each a match between two groups made up of three standard colours, and a fourth colour different for each observation, the results may be regarded as expressing the ( $n$ ) fourth colours as linear functions of the

standards. If, now, the theory be true, the new colour-equations which we should obtain from the former by mere algebraical elimination, ought to agree with the results of direct observation on the matches indicated thereby. Professor Maxwell has subjected the theory to this verification, and thereby raised the composition of colours to the rank of a branch of mathematical physics.

Not the least remarkable part of these researches consists in their application to the case of colour-blindness. All the phenomena of colour-matches, as seen by the colour-blind, can be represented quantitatively as well as qualitatively, as Professor Maxwell has shown, by supposing that, as seen by such persons, all colours may be expressed in a linear manner by means of two chosen at pleasure. The results, as he has pointed out, would seem to indicate that the normal-eyed have (as Young supposed) three primary colour-sensations, and the colour-blind but two.

These researches Professor Maxwell has recently completed, by a paper published in the first part of the 'Philosophical Transactions' for the present year.

The researches for which the Rumford Medal is awarded, lead to the remarkable result that, to a very near degree of approximation, all the colours of the spectrum, and therefore all colours in nature which are only mixtures of these, can be perfectly imitated by mixtures of three actually attainable colours, which are the red, green, and blue, belonging respectively to three particular points of the spectrum.

#### MR. PROFESSOR MAXWELL,

In presenting you with this Medal, I may express the pleasure with which the Council, and I may add, I am sure, the Fellows of the Society generally, have seen your recent removal to London; and their hope that the 'Transactions' may be frequently enriched by contributions from one who already, in this early part of his career, has attained so much distinction.

The Statutes relating to the election of Council and Officers having been read, and Mr. Glaisher and Mr. Smee having been, with the consent of the Society, nominated Scrutators, the votes of the Fellows present were collected.



The following Gentlemen were declared duly elected Council and Officers for the ensuing year :—

*President.*—Sir Benjamin Collins Brodie, Bart., D.C.L.

*Treasurer.*—Major-General Edward Sabine, R.A., D.C.L.

*Secretaries.*— { William Sharpey, M.D., LL.D.  
George Gabriel Stokes, Esq., M.A., D.C.L.

*Foreign Secretary.*—William Hallows Miller, Esq., M.A.

*Other Members of the Council.*—John Couch Adams, Esq.; Sir John Peter Boileau, Bart.; Arthur Cayley, Esq.; William Fairbairn, Esq., LL.D.; Hugh Falconer, M.D.; William Farr, M.D., D.C.L.; Thomas Graham, Esq., M.A., D.C.L.; Sir H. Holland, Bart., M.D., D.C.L.; Thomas Henry Huxley, Esq.; Sir J. G. Shaw Lefevre, M.A., D.C.L.; James Paget, Esq.; Joseph Prestwich, Esq.; William Spottiswoode, Esq., M.A.; John Tyndall, Ph.D.; Alex. William Williamson, Ph.D.; Col. Philip Yorke.

The following Table shows the progress and present state of the Society with respect to the number of Fellows :—

	Patron and Honorary.	Foreign.	Having com- pounded.	Paying £2 12s. annually.	Paying £4 annually.	Total.
December 1, 1859..	7	47	353	7	277	691
Since elected .....	.....	+4	+6	.....	+16	+26
Since deceased ....	.....	—2	—17	.....	—18	—37
Withdrawn .....	.....	.....	.....	.....	—2	— 2
Defaulter .....	.....	.....	.....	.....	—1	— 1
November 30, 1860	7	49	342	7	272	677

*Receipts and Payments of the Royal Society between November 30, 1859, and December 1, 1860.*

	£	s.	d.
Balance .....	420	0	0
Subscriptions and Compositions .....	1634	4	0
Rents .....	238	8	6
Dividends on Stock, including Trust Funds .....	1175	6	11
Sale of Transactions, Proceedings, &c. ....	380	8	0
Compensation for Rights of Common at Acton .....	41	2	10
Stevenson Bequest .....	783	18	5
Chemical Society, Subscription to 'Proceedings' .....	20	0	0
Ditto, Tea Expenses and Lighting .....	17	4	6
Linnean Society, Tea Expenses and Lighting .....	17	4	6
Geographical Society, Lighting Hall .....	13	10	0
Geological Society, ditto .....	4	0	0

*Estates and Property of the Royal Society, including Trust Funds.*

Estate at Mablethorpe, Lincolnshire (55 A. 2 R. 2 P.), £116 10s. per annum.  
 Estate at Acton, Middlesex (34 A. 3 R. 11 P.), £110 0s. 0d. per annum.

Fee farm rent in Sussex, £19 4s. per annum.  
 One-fifth of the clear rent of an estate at Lambeth Hill, from the College of Physicians, £3 per annum.  
 £14,000 Reduced 3 per Cent. Annuities.  
 £26,476 19s. 1d. Consolidated Bank Annuities.  
 £513 9s. 8d. New 2½ per Cent. Stock.

*Scientific Relief Fund.*

Investments up to August 7th, 1860, New 3 per Cent. Annuities, £4824 9s. 9d.

EDWARD SABINE,  
Treasurer.

£4745 7 8

	£	s.	d.
Rev. T. Evans, Fairchild Lecture .....	2	16	3
Mr. Fairbairn, Bakerian Lecture .....	3	18	4
Mr. J. Pettigrew, Croonian Lecture .....	2	18	9
Salaries, Wages, and Pension .....	1033	0	0
Fire Insurance .....	42	1	6
Ditto Proceedings and Miscellaneous .....	533	14	10
Printing Transactions, Parts 1 & 2, 1859, and Part 1, 1860.	286	19	1
Engraving .....	267	3	1
Paper for Transactions and Proceedings .....	521	8	9
Binding Transactions .....	99	12	3
Books Purchased and Binding .....	280	19	0
Furniture .....	130	17	4
Stationery .....	21	18	1
Shipping Expenses .....	6	2	0
Fire and Lighting .....	108	17	4
House Expenses .....	63	4	6
Tea Expenses .....	58	13	11
Taxes .....	30	17	6
Donation Fund .....	232	2	6
Wintringham Fund .....	35	5	0
Copley Medal Fund .....	4	17	7
Bunford Medal Fund .....	140	16	7
Catalogue of Scientific Periodicals .....	185	17	0
Postage, Miscellaneous and Petty Charges .....	83	8	0
Acton Estate Enclosure Expenses .....	69	6	6
Catalogue of Portraits .....	21	0	0
Balance at Bankers .....	497	12	0

£4745 7 8

*Statement of Income and Expenditure (apart from Trust Funds) during the Year ending November 30, 1860.*

	£	s.	d.		£	s.	d.
Annual Subscriptions .....	£1134	4	0	Salaries, Wages, and Pension .....	1083	0	0
Admission Fees .....	200	0	0	Fire Insurance .....	42	1	6
Compositions .....	300	0	0	Printing Transactions, including Part 1, 1859 .....	2533	14	10
Rents .....				Ditto Proceedings and Miscellaneous .....	286	19	1
Dividends on Stock (exclusive of Trust Funds) .....				Engraving .....	267	3	1
Sale of Transactions, &c. ....				Paper for Transactions and Proceedings .....	521	8	9
Stevenson Bequest.—Portion of Residue and Income .....				Binding Transactions .....	99	12	3
Chemical Society, Subscription to 'Proceedings' .....				Books Purchased .....	194	12	2
R. Geographical Society, for Lighting of Hall .....				Binding Books .....	66	6	10
Linnean Society, Tea Expenses and Lighting .....				Furniture .....			
Chemical Society, ditto .....				Stationery .....			
Geological Society, Lighting Hall .....				Shipping Expenses .....			
Income available for the Year ending Nov. 30, 1860 .....	4017	2	8	Fire and Lighting .....			
Expenses in the Year ending Nov. 30, 1860 .....	3825	0	8	House Expenses .....			
				Tea Expenses .....			
				Taxes .....			
Excess of Income over Expenditure in the Year ending	£192	2	0	Catalogue of Scientific Periodicals .....			
Nov. 30, 1860 .....				Catalogue of Portraits .....			
				Acton Estate Enclosure Expenses .....			
				Postage, Miscellaneous and Petty Charges .....			
					£3825	0	8

In the similar account to the present, rendered Nov. 30, 1859, it was stated that the Society was liable for the Expenses of Printing, Paper, and Engraving the Transactions (Part 1, 1859). This liability, amounting to £550, has been paid off, and is included in the disbursements of the present year. In addition to Part 1, 1859, two Parts, viz. Part 2, 1859, and Part 1, 1860, have been printed and paid for in the current year. There is therefore now no liability; and the sum of £550, for which the Society was liable at the last Anniversary, though paid in the present year, does not properly form a part of its expenses, and being added to £192 2s., the excess of income over payments, makes £742 2s. as the true excess of Income over Expenditure in the year which is now closed.

EDWARD SABINE,  
Treasurer.

*December 6, 1860.*

**Major-General SABINE, Treasurer and Vice-President,  
in the Chair.**

The Chairman announced that the President had appointed the following Members of the Council to be Vice-Presidents :

**The Treasurer.**

**Sir John P. Boileau, Bart.**

**Thomas Graham, Esq.**

**Sir Henry Holland, Bart.**

The Earl de Grey and Ripon was admitted into the Society.

In accordance with Notice given at the last Ordinary Meeting, the Right Hon. George Granville Francis Egerton, Earl of Ellesmere, was proposed for Election and immediate ballot.

The ballot having been taken, his Lordship was declared duly elected.

The following communications were read :—

- I. "On Coal-Gas." By the Rev. W. R. BOWDITCH. Communicated by Professor WILLIAM THOMSON. Received June 10, 1860.

A distinguished Fellow of the Royal Society discovered coal-gas, when Rector of Crofton, about two miles from my present parish, and nearly all our knowledge of this complex substance is derived from the labours of chemists who have been, or are, Fellows of the Society. I feel assured, therefore, that an attempt to extend the knowledge of the reaction of coal-gas with various substances will be favourably received, and that the application to practice of the facts made known, will not render a memoir less acceptable to the Society which rewarded alike the abstract researches of Leverrier and the practical ones of Arnott.

Six years ago I introduced the use of clay into gas-works, for the purpose of improving the purification of coal-gas, and now,—after so long an experience, the purification of many hundreds of millions of

feet of gas, and the use of many thousand tons of the refuse as manure,—I venture, for the first time, to submit the grounds upon which my process is based.

Coal-gas may conveniently be considered under the heads of carbon compounds required for the production of heat and light, which generate water and carbonic acid by their combustion; and sulphur and nitrogen compounds which are not necessary for heat and light, and ought to be removed from gas on account of the injurious nature of the substances produced by their combustion.

The former of these classes will be treated of incidentally; the latter class forms the principal subject of this paper. When speaking of *gas*, I always refer to that which has undergone the ordinary condensation of gas-works, wherefore no mention is made of the complex compounds removed by condensation.

When coal is distilled, its nitrogen is evolved in some forms of combination which are generally familiar, while others are almost unsuspected. Under certain conditions of distillation, much nitrogen leaves the retorts and passes the condenser as ammonia or some of its salts. These are all removed from gas by clay, so that no trace of ammonia can be discovered after gas has passed through purifiers charged with an adequate quantity of clay, and with lime or some equivalent substance to remove sulphide of hydrogen. Clay is thus entitled to be classed with acids and some metallic salts as a purifier of gas, for these, of course, remove ammonia and its salts. But between clay and acids there is an important difference, in regard to the action which takes place upon the most valuable light-giving constituents of the gas; acids remove a large quantity of these, clay does not. We have experimental proof that clay does not remove the valuable hydrocarbon vapours from gas, in the fact that strong spirit of wine digested upon foul clay for days, does not thereby become much more luminous than it was before being so treated. The very slight light-giving power which it has obtained is due to tar; for if the spirit be evaporated, and the tar so obtained be redissolved in fresh spirit, the same kind of flame will be obtained as before; whereas the addition of a small portion of coal-oil to spirit gives a flame of considerable illuminating power. To this I may add, that long and extensive experience shows that the employment of clay in the purifying process improves the light-giving power of gas, by

removing substances which are not otherwise removed, and which, if allowed to be burnt with the gas, lessen its illuminating power. These light-damaging compounds are produced during the later portion of the distillatory process, as I have proved by experiment. The same retort was charged twice with the same weight of the same coal. The gas produced by one charge was purified by lime only, that produced by the other charge was purified by lime and clay. The illuminating power of the gas passing at each half hour's end was determined, and it was found that the purification made no difference for the first three or four half-hours. About the middle of the charge, that purified by my process had slightly the advantage, and at the close the difference in favour of that purified by the addition of clay has been found as much as ten or twelve per cent. Thus it is shown that the compounds removed by clay from gas produced during the early stages of distillation—however objectionable on other accounts—do not lessen the light-giving power of gas, whereas those removed during the later periods of distillation reduce the light-giving powers considerably.

If conjecture be allowable, I would venture an opinion that cyanogen compounds, and other nitrogenized substances with which foul clay abounds, are those which lessen light. My own investigations lead directly to this inference, and, I think, explain an old Table by Dr. Henry in this sense. In the 'Philosophical Transactions' for 1808, he shows that the gas produced from 112 lbs. of Cannel-coal contained, after purification, the following quantities of nitrogen :—

Hours from commencement of distillation.	100 measures of purified gas contain measures of nitrogen.	
$\frac{1}{2}$ an hour...	20	Due chiefly to atmospheric air. Probably the time when ammonia was principally produced. Probably vapour of water was present in very small quantity, and cyanogen and related compounds were produced in increasing quantity.
1 hour .....	42	
3 " .....	5	
5 " .....	15	
7 " .....	15	
9 " .....	15	
10 $\frac{1}{2}$ " .....	20	
12 " .....	20	

Without assuming the absolute accuracy of these figures, we may regard them as valuable indicators, pointing, I think, in the direction I have ventured to conjecture.

A beautiful reaction furnishes experimental proof of the damage

done to gas by acids. Clean deal sawdust is well moistened with pure sulphuric acid, diluted with five or six volumes of water, so that the sawdust may not be discoloured, and gas is passed through it in a slow stream. With rich gases, which give the light of from 20 to 25 sperm candles for a consumption of five feet an hour, the sawdust instantly changes to a most beautiful pink colour, and the tint gradually deepens until the whole becomes of a dark mahogany. With poor gases, which give the light of from 10 to 12 candles, this coloration is exceedingly faint at first, and deepens very slowly. The differences of coloration are so considerable and constant, that I have no doubt of the possibility of thus determining the value of gas as an illuminant. By using a standard acid, the same kind of sawdust, a uniform volume of gas, and the same sized U-tubes, notation of time and depth of colour would give a close approximation to the illuminating value of the gas. At all events, the sources of error are not greater than those of photometry in the hands of all but the most experienced, and the process is quite as close an approximation to truth as an ultimate analysis of gas, containing, as it does, impurities which render skill and precaution useless. A comparison of the analysis of coal-gas given in 'Bunsen's Gasometry,' with the substances now known to exist in gas, will convince us that at present we cannot attach any value to such analyses.

To determine the substances in gas which produce this coloration, some of its chief illuminating constituents were prepared and passed separately through the acid sawdust.

Olefiant gas made in the usual manner, and carefully purified, reddens the acid sawdust. Ether vapour does not affect it, and therefore need not be removed from the gas for this experiment.

Propylene, produced by passing the vapour of fusel-oil through a red-hot combustion-tube filled with cast-iron nails, but kept at so low a temperature that a small portion of oil passed over without decomposition, reddened the acid sawdust.

Commercial benzole, with the exception of one specimen, reddened the acid sawdust.

I have not yet had leisure to prepare and test acetylene.

The coloration of fir-wood, moistened by hydrochloric acid, has been mentioned by Williams as characteristic of pyrrole.

To show that the colour was produced by illuminating matter

abstracted, some sawdust was treated with acid strong enough to char it slightly\* ; and gas, which instantly reddened the clean sawdust and dilute acid, was passed first through the black and then through the clean acid sawdust. No colour was produced in the latter, though the flow of gas continued for an hour.

Hydrochloric may be substituted for sulphuric acid, so far as that gas colours sawdust moistened with it, but it is liable to a considerable disadvantage. If gas contain ammonia, the vapour of the acid unites with it in the tube before the gas comes into contact with the sawdust, and the result is a deposit of chloride of ammonium on the surface of the sawdust where the colour commences, which renders the observation less precise and easy. Olefiant gas likewise does not redden this acid sawdust, and therefore cannot be estimated by it.

Nitrogenized compounds in coal-gas present the greatest difficulty in the way of efficient purification, and the almost impossibility of obtaining them in a state fit for examination, renders their investigation laborious and unsatisfactory. Much nitrogen is contained in gas as cyanogen, which can be separated from the clay used in purification. Probably not much less exists as sulphocyanogen, which can be separated from the foul clay with ease, and the presence of further quantities in combination with sulphuretted hydrocarbons and tar can be demonstrated. The bodies formed by this combination of elements are, I believe, unknown at present.

By placing clay in a purifier through which crude gas passes from the condenser of a gas-works, and treating the saturated clay with spirit, a solution is obtained, of a brown colour, which has no effect upon litmus, turmeric or lead-paper, which decolorizes a solution of iodine, and from which nitrate of silver throws down a white or brownish white precipitate, and acetate of lead a white precipitate. The aqueous solution possesses the same properties, and, like the solution in spirit, is always neutral. Litmus paper, immersed in either of the solutions and exposed to the air, becomes quickly, strongly, and permanently reddened. Soluble sulphides have been tested for repeatedly with nitroprusside of sodium, as well as with acetate of lead, but have never been found ; yet a sulphur compound exists in solution which possesses the power of forming a sulphide with

\* This acid was of the same strength as that used in some gas-works.



metallic mercury. The spirit solution, digested on mercury, with occasional shaking, produces the black sulphide of mercury, while the aqueous solution, similarly treated, produces the red sulphide. Insoluble sulphides, however, exist in the foul clay, and evolve sulphide of hydrogen on the addition of an acid. These insoluble sulphides are oxidized rapidly by exposure of the clay to atmospheric action.

A solution of clay in spirit was treated with an excess of powdered acetate of lead, and the white precipitate filtered off. The brown filtrate was supersaturated with ammonia and filtered. The clear brown filtrate, diluted with twelve times its bulk of water, became milky, and with much difficulty was obtained clear by filtration. Part of the spirit was then distilled off, to ascertain whether it would bring over a volatile sulphur compound, but the spirit was quite free from sulphur. The remaining fluid was then acidified with nitric acid, which caused brisk effervescence and a strong smell of hydrocyanic acid. Nitrate of silver was added as long as it continued to produce a precipitate; the precipitate, dried and heated, gave off cyanogen, which burnt with its characteristic flame. The clear filtrate, slowly evaporated to dryness, left a pale yellow crystalline mass, which did not change colour by several days' exposure to light. Part of this, burnt in a porcelain crucible, gave off nitrous fumes, and left a considerable residue blackened by oxide of silver. Water was added to this residue, and the oxide of silver filtered off, and an abundant precipitate of sulphate of baryta obtained, with a salt of baryta. The remainder of the yellow salt was redissolved in water, with a view to separate a granular portion which was mixed with the more perfectly crystalline salt, but an accident unfortunately spoilt the remainder, and rendered any further progress impossible.

Sulphocyanide of ammonium may be obtained in considerable quantity from an alcoholic solution of foul clay. Upon one occasion I obtained nearly an ounce, in a fair state of purity, from less than a quart bottle of foul clay; and so tenaciously does clay retain this compound, that from some clay which had been exposed to the full action of the weather in a field for two years, I obtained a considerable coloration with perchloride of iron. Sulphocyanide of ammonium may be obtained from gas which has been purified by oxide of iron, by passing the gas through spirit of wine and evaporating.

When common yellow brick-clay is used in the purification of coal-

gas, the solutions from it always contain salts of iron, but they never become of a blood-red colour until a mineral acid is added. When, however, the solutions are evaporated, and the deliquescent residue is exposed to the air, most, and sometimes all of the iron is peroxidized and yields the well-known reaction.

The nitrogen in tar may be shown from the spirit-solution off foul clay. The spirit, evaporated to dryness and allowed to stand, deposits tar and a mixture of deliquescent crystalline salts. They were allowed to deliquesce, the fluid was removed, and the residual tar well washed with water. Subsequently it was dissolved in hot spirit, precipitated by water, and well washed. When nothing more was removed by washing, the tar was heated, and evolved sulphide of hydrogen and ammonia. Contrary to every other compound in gas with which I have experimented, this tar gave off sulphide of hydrogen before ammonia. In other instances I have found the nitrogen compound evolved at a lower temperature than the sulphur one.

Mineral matter derived from the clay is found in all solutions; but as my object in this paper is to speak only of substances in gas, I purposely omit those united with them derived from the clay. For the same reason I make no mention of the value of the foul clay as a manure.

*Sulphur compounds in gas purified so as not to affect basic acetate of lead, and their removal.*

A recent Royal Commission on lighting picture galleries, has stated the large quantity of sulphur found in some London gas, and has intimated a doubt about the possibility of its removal. Dr. Letheby concludes, from seven years' examination of gas in London, that though quite free from sulphide of hydrogen, it contains, on an average, 200 grains of sulphur in 1000 cubic feet; and Dr. Frankland, in the new edition of 'Ure's Dictionary of Arts,' part iv. pp. 730, 731, writes, "It is probable that volatile organic compounds of sulphur are produced by the action of this element with carbon and hydrogen simultaneously, although we have as yet no positive evidence of their presence in illuminating gas. . . . When once generated with coal-gas, all attempts to remove these constituents have hitherto proved ineffectual, and there seems little ground for hope that any practicable process will be devised for their abstraction." I have now the honour to submit evidence of the existence of these sul-

phurized compounds, and also a practical process for their removal. My attention was specially drawn to this subject by a conversation with the manager of a London gas-works. He informed me that he not unfrequently filled his gas-holders with gas which would not affect acetate of lead, and that after the gas had been stored a few hours, it became so foul as to blacken lead-paper the instant it was applied. He sought an explanation of this phenomenon ; and as the water of his gas-holder tanks was clean, and there were no accidental sources of sulphide of hydrogen, I concluded that an organic compound containing sulphur and hydrogen had been broken up, and that the sulphide of hydrogen was thus produced. I learnt also, by other observations, that gas which went to the gas-holders free from ammonia, sometimes became ammoniacal if kept, and joining this fact with the former one, inferred that the compound which thus broke up contained nitrogen as well as sulphur and hydrogen. Subsequently I observed that the saturated clay taken from the purifiers of gas-works, contained a quantity of foul naphthalin. This led me to procure a quantity of (so called) naphthalin which had been taken from the mains of a London gas-works, and which therefore must have been deposited by purified gas. Some portions of this naphthalin were white, but others were slightly darkened by the presence of carbonaceous matter, and the whole was in *fine powder*, aggregated together by the process of deposition. The tendency to form exceedingly small crystals seems a constant characteristic of naphthalin which has been deposited in gas-pipes, for by no amount of care and trouble have I been able to obtain it in large crystals, though the solutions from which it has crystallized have been months in evaporating. With naphthalin from tar, on the contrary, I have obtained, from an etherial solution, crystals an eighth of an inch thick, nearly half an inch broad, and more than half an inch in length. The supposed naphthalin from gas-pipes dissolves wholly in ether and hot alcohol, and crystallizes from the spirit on cooling as pure naphthalin does. The solutions are neutral to test-papers. Boiled with an alcoholic solution of potash it evolves no ammonia, and with hydrochloric acid no sulphide of hydrogen. Heated alone, it evolves first ammonia, and then sulphide of ammonium, mixed, I think, with a trace of bisulphide of carbon, and then distils. Several samples began to give off their ammonia at 388° Fahr., and sulphide

of hydrogen at  $422^{\circ}$  Fahr., leading to the hope that here was a compound of definite composition which would admit of correct analysis and perhaps of formulation ; but some more of the naphthalin, produced, like the other, from Newcastle coal, but at another gas-works, possessed such different physical properties as to convince me that very much more must be known of this substance before any reliable analyses can be published. The latter sample gave a neutral solution in spirit, like the other, but of a considerably browner colour. When heated alone it gave off ammonia with ebullition at  $218^{\circ}$  Fahr., and then became tranquil. When the temperature reached  $375^{\circ}$  Fahr., it began to evolve sulphide of hydrogen, which continued to increase in quantity up to  $390^{\circ}$  Fahr., when it nearly ceased, and quite ceased at  $410^{\circ}$  Fahr. The proportion of tar in this sample was much greater than I have seen it in any other. Subsequently I obtained some more naphthalin which had been deposited in the pipes of another London gas-works, and this, like the former, contained both nitrogen and sulphur, which were evolved upon distillation as sulphide of hydrogen and ammonia.

Having thus obtained one sulphurized hydrocarbon, and determined the temperature at which its sulphur and nitrogen could be obtained as easily removeable compounds, I was prepared to advance towards a better purification of gas with great probability of success. Another well-confirmed observation helped to guide me. Gas freed from every trace of sulphide of hydrogen always blackens lead-paper strongly when passed through clay ; and if it be subsequently passed through lime, it affects turmeric though quite free from ammonia when taken for experiment. This process may be repeated through a series of ten or twelve purifiers containing clay and lime placed alternately, the test-papers being less affected at each purifier, until at length they are not discoloured at all. This experiment has been made upon gas produced in various parts of England and Scotland from many kinds of coal, and I think the number of instances sufficient to justify the conclusion that *all* gas, as sold, contains the compound from which clay liberates sulphide of hydrogen. I have not yet been able to separate the compound upon which clay thus acts. I have, however, ascertained that the clay which has liberated sulphide of hydrogen from gas which did not affect test-papers when taken for experiment contains tar, which may be dis-

solved out by alcohol, and may be obtained alone by evaporating the solvent.

Although collateral matter has been carefully excluded from this paper, I cannot refrain from remarking that the property of clay here mentioned is in fact the announcement of a new property of soils, and one which will help to account for the formation of many natural metallic sulphides. I hope soon to have some investigations of this subject ready for publication.

To ascertain whether this property of breaking up a sulphurized compound in purified gas and removing tar was possessed by clay alone, or shared by other substances used in purification, some purified gas was passed through a considerable chemical excess of all the substances employed in purification, viz. lime, precipitated peroxide of iron, sulphate of iron, chloride of calcium, and dilute sulphuric acid, all but the lime being mixed with moist sawdust. Upon passing the gas next through a purifier filled with clay, it darkened lead-paper, and affected turmeric when it had passed a subsequent purifier filled with lime. This proves the power of clay to break up one or more sulphurized compounds which no other substance used in purification effects; and if this sulphur were not liberated from the impure naphthalin compound already mentioned, it seemed certain that gas which had been previously purified by clay might be much improved, if not rendered pure, by a removal of the sulphur of the naphthalin. There is strong experimental evidence that the compound from which clay liberates sulphide of hydrogen is not the sulphurized naphthalin one; for if hydrogen be passed through a vessel containing this substance, then through clay, and subsequently over lead-paper, no trace of sulphide of hydrogen is found, though the gas passing smells strongly of impure naphthalin. This gas and vapour burn with a lightless flame. Subsequently some naphthalin was heated to ebullition, and a current of hydrogen sent through it and then burnt. The flame was lightless as before. I mention this fact to remove the popular error that naphthalin, as it exists in coal-gas, is a good illuminant. Even Dr. Frankland thus regards it; and both in Clegg's book on coal-gas, and in the new edition of Ure's Dictionary, states that the hydrocarbons in gas are valuable in proportion to the carbon they contain, and that naphthalin is the most valuable as containing the largest proportion of carbon. The

above-mentioned experiments evince the contrary. On another occasion I determined the illuminating power of some gas, and then, without alteration of the quantity passing, sent the gas through a U-tube containing naphthalin from the London gas-mains. The character of the flame was changed from white to red, but the photometer indicated no difference in the light given. Two other persons conversant with photometry were present at this experiment and agreed in the result, though up to that time they had held the prevailing opinion as to the value of naphthalin in gas without testing the statements made upon the subject.

Another sulphur compound is said always to be present in coal-gas and to be irremovable, and which, like those I have hitherto spoken of, does not affect lead-paper, viz. bisulphide of carbon. To ascertain the presence of bisulphide of carbon, I pass gas through strong spirit of wine (methylated spirit answers perfectly) kept at about 160° Fahr. The gas and vapours pass out of the flask which contains the spirit up a long tube into an inverted flask, so that all which is condensed may run back into the spirit. It then passes into another flask for additional condensation, and thence forward to a gas-holder or burner. Bisulphide of carbon dissolved in spirit becomes precipitated as a white cloud which settles to the bottom of the vessel, when the spirit is copiously diluted with water. The white cloudy precipitate escapes slowly by single bubbles through the diluted spirit, and at length leaves a solution perfectly clear. The spirit through which gas has passed, and from which it has abstracted bisulphide of carbon, acts in precisely the same manner upon dilution, and no one who has seen the reaction once or twice can possibly mistake it. The study of other compounds led me to conclude that if this substance exist *as such*, and not merely *by its elements*, in gas, it could be removed almost as easily as the naphthalin compound could be purified, and that the same process could be made available to remove the sulphur of both. I thought that under certain conditions the affinity of hydrogen for sulphur would exceed that of carbon for sulphur, and therefore that I might obtain the sulphur of bisulphide of carbon as a sulphide of hydrogen, about the removal of which there is no difficulty. Experiment confirms the reasoning. When hydrogen mixed with vapour of bisulphide of carbon is passed through a tube filled with slaked lime or clay

which has been dried at 400° or 500° Fahr., and is kept between 400° and 600° Fahr. during the passage of the gas and vapour, not a trace of bisulphide of carbon passes from the tube, but sulphide of hydrogen does pass. The lime is darkened by a deposit of carbon, and a sublimate of sulphur is found in the exit-tube. A considerable excess of hydrogen should be used, or else a portion of the bisulphide of carbon vapour is carried over by the current and escapes decomposition. That this reaction is not the result of heat merely, but is a truly chemical one which the base under the influence of heat effects, and the remarkable fact that slaked lime when heated forms, but does not unite with, sulphide of hydrogen, receive illustration from the following experiments.

Hydrogen and vapour of sulphide of carbon were passed through—1st, cold slaked lime; 2nd, cold clay; 3rd, hot oxide of iron used at a gas-works in purification; 4th, hot broken bricks; 5th, hot broken glass, without in any instance producing sulphide of hydrogen. On the contrary, when passed through (1) hot lime and (2) hot clay, sulphide of hydrogen was formed and passed over immediately, and continued to pass as long as the current was kept up. The lime, when cooled out of contact with the air, gave no sulphide of hydrogen upon being supersaturated with dilute sulphuric acid, but clay when thus treated gave off much.

The hydrogen used was in all cases passed through lime and over lead-paper, to secure its being free from sulphide before use. On one occasion, when the clay had been imperfectly dried before heating, I observed much sulphurous acid instead of sulphide of hydrogen. I therefore passed hydrogen, bisulphide of carbon vapour, and steam over hot clay which had been properly dried. At first sulphide of hydrogen passed over alone, then mixed with sulphurous acid, which at length passed alone. Subsequently sulphide of hydrogen passed, and at length sulphurous acid ceased. As the one gas increased the other diminished, and throughout the experiment sulphide of carbon vapour passed undecomposed. It is shown by this experiment that hot clay in the presence of more water than forms a hydrate acts very differently from the same clay when dry, and the whole subject deserves a full investigation.

*Action of sulphide of hydrogen upon clay and lime, cold and hot.*

Well-washed sulphide of hydrogen passed into cold slaked lime (obtained from Buxton) in a tube, colours the lime green as soon as it comes into contact with it, and the progress of the gas along the tube corresponds with the coloration. Lead-paper is not affected until the lime becomes coloured close up to the exit.

The same gas, passed into a tube containing slaked lime kept about 600° Fahr. at the middle, but cool at both ends, acts differently. The cool lime at the inlet end becomes coloured; the hot lime in the middle remains white, and the cool lime at the exit end becomes coloured, and lead-paper is stained as soon as these two cool portions are saturated, while the middle portion remains unchanged in colour.

The same gas, passed into a tube containing hot lime only, causes no discoloration, but instantly blackens lead-paper placed at the exit end; and upon being conducted into a tube of cold lime, colours it as if it had just passed from the vessel in which it is produced.

The same gas was passed into a tube containing lime which had been thoroughly dried at 600° Fahr., and cooled out of contact with the air. No discoloration of the lime took place, but the gas passed unaffected by the lime, and blackened lead-paper. Water added to the lime gives it the power of decomposing the gas as if it had not been heated. The presence of more water than is necessary to form hydrate of lime ( $\text{CaO}$ ,  $\text{HO}$ ), is thus shown to be required for the decomposition of sulphide of hydrogen by slaked lime.

Sulphide of hydrogen passed into a tube of cold clay is taken up in considerable quantity, and the clay becomes black from the formation of sulphide of iron. The blackening begins at the inlet end, and progresses with the passage of the gas towards the exit end of the tube.

The same gas, passed into clay, heated to 500° or 600° Fahr., gives the same reactions; but when the clay has been heated and well-dried, and cooled in the closed tube, it takes up a very small quantity of the gas.

Coal-gas, quite free from sulphide of hydrogen, when passed through hot lime, blackens lead-paper, showing that masked and hitherto irremoveable compounds have been so altered as to be easily removeable. The lime does not take up sulphide of hydrogen, but



becomes gradually, yet very slowly, darkened by the deposition of tar and carbon from vapour of bisulphide of carbon. The reaction with previously dried slaked lime commences at  $108^{\circ}$  Fahr., and continues through the whole range of temperature up to redness. At a red heat the sulphur of the bisulphuret of carbon and other sulphur compounds unites with the lime and forms sulphide of calcium. Practically very high temperatures are useless, as the hydrocarbons of gas begin to be decomposed about the melting-point of lead, and to deposit their carbon upon the hot lime. Fortunately injurious temperatures are not required. I have frequently freed gas from every trace of sulphur, so that upon combustion no sulphurous acid was generated, by employing lime so heated as not to deposit any carbon, and removing the sulphuretted hydrogen evolved in the hot tube by ordinary hydrate of lime.

The same gas passed through hot clay gradually darkens the clay by forming sulphide of iron, and, when the blackness has reached the end of the tube containing the clay, lead-paper is blackened by the passing gas. The clay treated with an acid evolves sulphide of hydrogen. Carbonic acid is evolved in both cases. It is thus proved that bisulphide of carbon, in the presence of hydrogen passing over hot hydrate of lime, is decomposed, and that its sulphur becomes united to hydrogen. Coal-gas always contains a considerable quantity of hydrogen, so that, if it contain vapour of bisulphide of carbon, the process I have the honour to describe will effect its removal. The same process will break up the impure naphthalin compound and convert its sulphur into sulphide of hydrogen; and the employment of clay in the ordinary purifiers, before the gas passes through the hot ones, will so arrange the elements of certain other sulphur compounds as to enable the manufacturer to remove their sulphur as sulphide of hydrogen. Sulphocyanide of ammonium is decomposed by the heated lime, and its sulphur is liberated as sulphide of hydrogen. The only requisite for complete success was that no injury should be done to the light-giving materials of gas while removing the impurities. I have passed the principal illuminating constituents of coal-gas through the hot lime and clay, and find that they are not injured. The temperature which suffices for purification is not high enough for injury. The photometer shows that coal-gas is not injured.

The quantity of tar in gas as supplied to consumers, and the evil of its presence as a source of sulphur, are not considered as, I think, they deserve to be. It is exceedingly rare to find gas free from tar, and I never yet met with tar which did not contain both nitrogen and sulphur. Part of this tar is combined with ammonia in some manner, and may be obtained by passing gas through a bottle containing pebbles moistened with hydrochloric acid. Part is united to naphthalin, as I have already mentioned : part is united to benzole vapour, and part to other hydrocarbon vapours, such as paraffin, if two instances within my own knowledge be sufficient to justify a statement in reference to gas in general. In one instance I passed gas through a metal vessel filled with a number of wire-gauze diaphragms and kept below 32° Fahr. Some cakes of solid paraffin were found floating upon the water which had been placed in the vessel before commencing the experiment, and a mixture of tarry oils which had deposited the paraffin. In another instance, an old gas-holder was about to be replaced by a new one, and on the water of the tank in which the old gas-holder had worked, there was found upwards of a thousand gallons of a dark-coloured fluid. All but two carboys was sold to a tar distiller. These two carboys were left exposed to the air without corks for some time, and when the manager of the gas-works went to get me some of the fluid for examination, he found that the whole contents had evaporated. I had previously, however, obtained about half an ounce of the mixture. It contained paraffin, naphthalin, and the oils which accompany paraffin. Nearly a fifth of its weight of solid pitch was obtained by distilling off the hydrocarbons. A quantity of sulphide of hydrogen and ammonia were evolved during the distillation, and some of the most stinking compounds I ever met with produced from coal. From these two instances it is clear that some, or perhaps all, of the volatile hydrocarbons in gas possess the power of upholding tar with them in their vapours ; and it is proved that this tar is no inconsiderable source of the sulphurous acid produced by the combustion of gas as at present purified. I have obtained tar containing sulphur from every specimen of commercial benzole I have examined ; and as this will evaporate at common temperatures without leaving a residue, we are justified in the presumption that tar thus united to benzole exists in gas.

The best method of showing the tar in gas is to pass it through or over well-purified coal-oil, and subsequently through a good condensing arrangement. I have known colourless coal-oil become of a dark mahogany colour, and have separated sulphuretted hydrogen, ammonia, and solid pitch by distillation.

II. "On the Gyroscope." By ARTHUR HILL CURTIS, Esq.  
Communicated by Professor BOOLE. Received Sept.  
20, 1860.

(Abstract.)

The object of this paper is to deduce on strict mechanical principles all the known properties of the gyroscope. The only assumption made is that the velocity of rotation impressed on the instrument is very great compared with that which the attached weight would produce on it if acting alone for an instant in a direction perpendicular to the axis. The theorems which the author establishes are the following:—

**THEOREM I.**—The curves described by the extremity of the axis of the gyroscope are a system of spherical cycloids generated by the motion of a point on the spherical radius of a circle, which, constantly remaining on the same sphere, rolls without sliding on the circumference of another fixed circle situated on the same sphere. These cycloids may be either ordinary, curtate, or prolate—including the case when the system degenerates into a circle, in which case the generating point becomes the centre of the rolling circle. Their species depends on the direction of the initial velocity communicated to the axis, the direction in which the instrument is set rotating, and the position of the attached weight; when, for instance, no initial velocity is communicated to the axis, the cycloids will be ordinary *at first*, and would continue so if the gyroscope were a perfect instrument for illustrating the motion of a body round a fixed point; but the inertia of the rings on which it is mounted, and of the attached weight, as well as the resistance of the air, after a short period has elapsed, has the effect of imparting to the axis a certain velocity which modifies the curves described by it, and at last causes the

motion of the axis to become for a time sensibly one of uniform progression ; it then becomes oscillatory again, the amplitudes of the oscillations being smaller than before.

**THEOREM II.**—If the outer ring be fixed in any position so as to restrict the axis of the gyroscope to a fixed plane, the motion of the axis, when a weight is attached as above, is the same whether the instrument be set rotating or not. It is proved that the angular motion of the axis is determined by an equation of the same form as that of a circular pendulum, which does not involve the angular velocity of rotation impressed on the gyroscope.

**THEOREM III.**—If the gyroscope be set rotating rapidly, and its axis of figure be constrained, as in Theorem II., to move very freely in a plane fixed with regard to the horizon, the axis will tend to take the position of the projection on the given plane of the line drawn through the centre of gravity of the gyroscope, parallel to the axis of the earth, in such a way that the earth and the gyroscope may turn in the same direction ; while, if the axis be perfectly free, it will move exactly in the same way as the axis of a telescope directed constantly towards the same fixed star, their initial positions being supposed parallel, as established experimentally by M. Léon Foucault (*Comptes Rendus*, September, 1852).

To prove this theorem, the angular velocity of the earth round its axis is resolved into an equal and codirectional motion of rotation round the line through the centre of gravity of the gyroscope parallel to the earth's axis, and a motion of translation, the direction of which is constantly changing, common to all parts of the earth. Of these motions the latter is communicated to the gyroscope by the friction of its base, and does not modify its position with regard to the horizon. The first alone requires to be considered. In order to estimate its effect, a rotation equal to it and round the same axis, but in an opposite direction, must be supposed to be communicated both to the earth and the gyroscope. This does not affect their relative motion, and simplifies the problem, as it enables us to consider the earth at rest. The relative motion of the gyroscope may therefore be found by adding to the three components, round its principal axis, of its instantaneous angular velocity of rotation, as found from its equations of absolute motion, the components of this introduced velocity of rotation, the moment of resistance of the given plane

being taken into account in forming the equations of motion, and its intensity supposed such as to counteract that part of the total angular velocity of the axis which is perpendicular to the given plane. The equation which determines the motion of the axis is shown to be identical with that of a circular pendulum, and the motion consequently one of oscillation, the mean position of the axis being that in which it approaches, as close as the conditions of the question permit, to the line drawn through its centre of gravity parallel to the earth's axis, and in which it rotates in a direction similar to that of the earth's rotation. Similar reasoning establishes the second part of the theorem, which is theoretically true whether the gyroscope be set rotating or not. This result is, however, in practice modified by the effects of friction; but when a rapid rotatory motion has been impressed on the gyroscope, it acquires a stability which enables it to overcome to a great extent these effects.

*December 13, 1860.*

Major-General SABINE, R.A., Treasurer and Vice-President,  
in the Chair.

The Right Hon. the Earl of Sheffield, and the Right Hon. Spencer Horatio Walpole were admitted into the Society.

The following communications were read:—

I. "On an Extension of Arbogast's Method of Derivations."

By ARTHUR CAYLEY, Esq., F.R.S. Received October 18, 1860.

(Abstract.)

Arbogast's Method of Derivations was devised by him with a view to the development of a function  $\phi(a + bx + cx^2 + \dots)$ , but it is at least as useful for the formation of only the literal parts of the coefficients, or, what is the same thing, the combinations of a given degree and weight in the letters ( $a, b, c, d, \dots$ ), the weights of the successive letters being 0, 1, 2, 3, &c. Thus instead of applying the method to finding the coefficients

$$a^4, 4a^3b, 4a^2c + 6a^2b^2, \&c.,$$

we may apply it merely to finding the sets of terms

$$a^4, a^3b, a^2c, \&c.$$

$$a^2b^2.$$

Nothing can be more convenient than the process when the entire series of columns is required ; but it is very desirable to have a process for the formation of any column apart from the others ; and the object of the memoir is to investigate a rule for the purpose. It is found that there is, in fact, a rule analogous in some measure to Arbogast's, but which consists in operating simultaneously upon certain pairs of letters : the pairs which may be operated on are four in number, but the conditions are such, that, as regards two of these pairs, when the one is operated on, the other is not, and for the same term the number of pairs operated on is therefore three at most.

II. " On the Method of Symmetric Products, and on Certain Circular Functions connected with that Method." By the Rev. ROBERT HARLEY, F.R.A.S., Corresponding Member of the Literary and Philosophical Society of Manchester. Communicated by ARTHUR CAYLEY, Esq. Received Oct. 18, 1860.

(Abstract.)

After briefly adverting to his own and to Mr. Cockle's earlier researches on the subject, the author proceeds in the first section of his paper to give a concise systematic exposition of the method of symmetric products, and to indicate a new application of the method to the solution of the lower equations. Hitherto, in applying the theory to the solution of cubics and quartics, an auxiliary equation has been introduced, the symmetric product of which equation has been made to vanish. The peculiarity of the process here employed, is that the auxiliary equation is dispensed with, and the symmetric product remains finite.

In the second section the author deals with circular functions. The structure of such functions is considered, and a calculus devised by means of which operations upon them may be materially abridged. The new cyclical symbol ( $\Sigma'$ ) is defined ; some of its applications are

given, and its power as a working instrument is illustrated by examples. By the help of this symbol, certain expressions which occur in 'endeavouring to extend the theory of symmetric products to quintics are greatly simplified and presented in an intelligible form, and the direct calculation is effected of a certain sextic equation on the solution whereof that of the general quintic may be made to depend.

The third and concluding section is chiefly occupied with the calculation of the perfect symmetric product for the quintic. By combining Eulerian with Lagrangian functions, and introducing a simple artifice, the symmetric product for the quintic wanting in its second term is obtained. This result is then made the basis of a calculation for the perfect form. Employing the property of seminvariancy pointed out by Mr. Cayley, the author succeeds in effecting the calculation of the symmetric product for the complete quintic. This product is composed of *three hundred and twenty-five functions of twenty-four dimensions*.

### III. "On the Surface-condensation of Steam." By J. P. JOULE, LL.D., F.R.S. Received Oct. 10, 1860.

(Abstract.)

In the author's experiments steam was passed into a tube, to the outside of which a stream of water was applied, by passing it along the concentric space between the steam-tube and a wider tube in which the steam-tube was placed. The steam-tube was connected at its lower end with a receiver to hold the condensed water. A mercury gauge indicated the pressure within the apparatus. The principal object of the author was to ascertain the conductivity of the tube under varied circumstances, by applying the formula suggested by Professor Thomson,

$$C = \frac{w}{a} \log \frac{V}{v},$$

where  $a$  is the area of the tube in square feet,  $w$  the quantity of water in pounds transmitted per hour,  $V$  and  $v$  the differences of temperature between the inside of the steam-tube, and the refrigerating water at its entrance and at its exit. The following are some of the author's most important conclusions.

1. The pressure in the vacuous space is sensibly the same in all parts.

2. It is a matter of indifference in which direction the refrigerating water flows in reference to the direction of the steam and condensed water.

3. The temperature of the vacuous space is sensibly equal in all its parts.

4. The resistance to conductivity must be attributed almost entirely to the film of water in immediate contact with the inside and outside surfaces of the tube, and is little influenced by the kind of metal of which the tube is composed, or by its thickness up to the limits of that of ordinary tubes.

5. The conductivity increases up to a limit as the rapidity of the stream of water is augmented.

6. By the use of a spiral of wire to give a rotary motion of the water in the concentric space, the conductivity is increased for the same head of water.

The author, in conclusion, gives an account of experiments with atmospheric air as the refrigerating agent; the conductivity is very small in this case, and will probably prevent air being employed for the condensation of steam except in very peculiar circumstances.

#### IV. "Notice of Recent Scientific Researches carried on abroad."

By the FOREIGN SECRETARY.

During the last Session of the Royal Society the Council passed a Resolution,—That it should be one of the duties of the Foreign Secretary to furnish the Society, from time to time, with early notice of researches of special importance carried on abroad; such notice to be drawn up in the form of a short communication to the Society, to be read as early as practicable at an Evening Meeting of the Society, and published in the 'Proceedings.' In the time which has since elapsed I have been only partially successful in obtaining that cooperation without which it is scarcely possible to comply with the instructions of the Council.



The regular and frequent appearance of the 'Comptes Rendus,' and of the Abbé Moigno's 'Cosmos,' containing, as they do, the earliest notices of important memoirs, and the publication in full of many memoirs in the 'Annales de Chimie,' render superfluous any mention of the labours of men of science in France. So also, very striking discoveries, such for instance as the qualitative analysis by the lines of the spectrum, and the consequent discovery of a new element, due to Professors Bunsen and Kirchhoff, published in the English Journals almost as soon as they are made, may be passed over in silence.

For most of the following notices I am indebted to the kindness of our Foreign Member, M. Haidinger, of Vienna. On receiving a letter containing the substance of the Resolution of the Council, he sent a printed circular, dated Sept. 1, 1860, to various active labourers in the field of science, inviting them to send a short account of the researches in which they themselves or others had been engaged, to the Foreign Secretary of the Royal Society. This appeal was responded to by the arrival of a number of letters containing a large amount of information which would otherwise have been most difficult to obtain.

The 'Denkschriften' and 'Sitzungsberichte' of the Academy of Sciences of Vienna are peculiarly rich in memoirs on Mineralogy, Crystallography, and on the optical and physical properties of Crystals, by MM. Haidinger, Zippe, Schabus, v. Zepharovich, Kengott, Hochstetter, Carl v. Hauer, v. Lang, Murmann, Rotter, Weiss, Handl, Dauber, Schrauf, Leydolt, Grailich, of whom the last two have been lately cut off by an early death. Many of these papers date back too far to be included in the present notice. Some of the most important of the recent communications on these subjects are by M. Haidinger, on Meteorites, on the form and optical characters of Hörnesite, a new mineral species belonging to the isomorphous group containing Vivianite, and several other hydrous arseniates and phosphates: by M. v. Zepharovich, who has also contributed many papers on Mineralogy, Geology, and Physical Geography, to the 'Jahrbücher der k. k. geologischen Reichsanstalt' of Vienna, on a new determination of the forms and angles of epidote, and some laboratory crystals not previously measured: by M. Carl v. Hauer, on Crystallogenesi, and on

compounds of selenium and vanadium : by M. Dauber, on the determination of crystallographic constants, and the limits of their errors : by M. Schrauf, on the form of Smithsonite ; and by M. Tschermak, on the secondary formation of minerals in the Greenstone of Neutitschein.

Major v. Sonklar is the author of a paper on the diminution of temperature at different heights, in different seasons of the year, in the Eastern Alps.

M. Kreil has determined the magnetic constants and geographical position of numerous points in the South-east of Europe, and some places on the coast of Asia.

M. Karl Friesach has continued the publication of his Magnetic and Astronomical Observations in North and South America.

M. Simon Spitzer has written a series of papers on the integration of linear differential equations.

MM. Helmholtz and v. Piotrowski are the authors of a memoir on the friction of incompressible fluids.

M. Karl Fritsch has written several memoirs on Meteorology. One of these is on the disturbances of the daily range of the most important meteorological elements on days in which thunder-storms occur. The daily range of atmospheric pressure, temperature, pressure of vapour contained in the atmosphere, from hour to hour, in different months, are known already for various stations. These, however, hold only for a mean condition of the atmosphere. In days on which thunder-storms occur, the range of each of the meteorological elements exhibits a peculiar character impressed upon it in the course of the preceding night and morning ; and, as thunder-storms usually occur in the afternoon and evening, the approach of a thunder-storm, or of the gale of wind by which it is not unfrequently accompanied, may in most cases be predicted. Another gives an account of the phænological observations made in the Austrian Empire in the course of the year 1858. This contains a comparison of the times of blossoming of seventy-three species of plants, at fifty-nine different stations, with the times of blossoming of the same plants at Vienna. On an average of the whole year, vegetation is earliest at Villa Carlotta on the lake of Como, 120 toises above the level of the sea, being fourteen days in advance of Vienna ; and latest at Gurgl in the Oetzthal, 966 toises above the

sea-level, being thirty-one days behind Vienna. This difference exhibits a regular diminution in each month from the spring to the summer.

The supplement to the eleventh volume of the 'Jahrbücher der k. k. Central-Anstalt für Meteorologie und Erdmagnetismus,' contains a comparison by M. Fritsch of those epochs in the life of plants and animals in different localities in the Austrian empire, which admit of being accurately determined for the year 1857. These observations include a record of definite phases of vegetable and animal life under the most various local circumstances, and extend as far as possible over the whole flora and fauna. They have been now continued for eight years. It is proposed to continue them till they embrace a period of ten years, when M. Fritsch hopes to deduce from them many important results, a task which will probably occupy him for several years. A memoir on the law of the influence of temperature on the epochs of definite phases of the development of plants, taking into account the effects of insolation and moisture, in the 'Denkschriften' of the Vienna Academy of Sciences, vol. xv., is intended as a precursor to this undertaking.

Professor Zantedeschi is the author of nine papers on Acoustics, published in the 'Sitzungsberichte' of the Academy of Vienna.

1. On the doctrine of the third sound, or on the coincidence of sonorous vibrations, with a remark on the analogy presented by the vibrations which constitute the light of the solar spectrum. The experiments of the author show that the number of vibrations in a given time, of the third sound, is always the difference of the numbers of vibrations of the two given sounds, and is not always the greatest common measure of the two given sounds, as has been asserted by some writers.
2. On the correspondence which sonorous bodies manifest in the resonance of many sounds in one.
3. On the unit of measure of musical sounds, on their limits, on the duration of the impression of the vibrations on the acoustic nerve of man, and on the increase of pitch of the fundamental note in tuning-forks of steel due to a spontaneous molecular change. The principal object of the author is the determination of a fixed note, to which, as to an invariable unit of measure, the notes of different instruments may be referred. The number of vibrations per second which produces the note C is from 272 to 276 at St. Petersburg; 271 in Naples; 268

in Milan, 266 in Venice, 268 in Vienna. According to M. Lissajous, in Paris, in 1856, the number of vibrations of the note A was 898; while according to Sauveur in 1715 it was 810. This elevation is by some supposed to be due to improvements made in the construction of musical instruments. Professor Zantedeschi attributes it to a molecular change which is gradually developed in the steel of which tuning-forks are made. In order to eliminate this source of error, he proposes to substitute for the tuning-fork a pipe such as is used at the present time by tuners of instruments in the South of Italy. He compared a number of tuning-forks and pitch-pipes known to be more than fifty years old, and found that the former had become higher, though unequally, when compared with the latter. In order to secure the fixity of a note, he considers that it ought to be compared with the syren of Cagnard de la Tour, or with the toothed wheel of Savart. By these means, especially when combined with improvements suggested by M. Zantedeschi, the stability of the note, or the amount of its error, may at any time be ascertained. In France the A has been lowered from 898 to 870 vibrations. 4. On the limits of the sounds produced by free reeds, and on their harmonics, considered in relation to the law of Bernoulli. 5. On the fundamental law of the harmonics of a musical string, on the vibratory motion from which they are derived, and on the interpolation of harmonics between the notes of instruments sounded by means of a bow and those of the human voice. 6. On the separation of waves corresponding to harmonics, and on the coexistence of several vibratory waves in the same aërial column. 7. On the lengths of the aërial waves, and on their velocity in pipes, and on the influence which various circumstances exert on the sound produced. 8. An experimental examination of the method commonly followed in the determination of the nodes and ventral segments of columns of air vibrating in tubes. 9. On the fundamental laws of vibrating rods, and of the oscillation of air in tubes.

Professor Zantedeschi is in possession of a series of meteorological observations embracing a very long period, by means of which he hopes to be able to establish the constants of the climate of Italy.

M. Schaub, Director of the Observatory, and Superintendent of the Hydrographical Institution at Trieste, is occupied with the discussion of tide observations made during one whole year, beginning

October 16, 1859, with a self-registering gauge, in the harbour of Trieste. The results of the observations for more than two months have appeared in the 'Mittheilungen' of the Geographical Society of Vienna. The height of high water above low water was found to be 87 centimetres at spring tides, and 24 centimetres at neap tides.

Professor Lorenz, of Fiume, the author of various Essays on Geology, Physical Geography, and Natural History of various districts in the Salzkammer Gut and on the Croatian coast of the Adriatic, which date back too far to fall within the scope of the present notice, has recently published the following memoirs:—Geological Survey of the Liburnian Karst and the adjacent Quarnero islands, giving a sketch of these Adriatic coast declivities and islands, the geological constitution of which had never before been examined with accuracy (*Jahrbuch der k. k. geologischen Reichs-Anstalt*, Band X.). An inquiry into the sources of the springs of the Liburnian Karst and adjacent Quarnero islands, some of which are remarkable for their excessively low and constant temperature (*Mittheilungen der k. k. geographischen Gesellschaft in Wien*, B. III.). On the forest-trees of the Liburnian Karst range. A Report on the question whether trees and other growths could be produced on the declivities of the Croatian Adriatic coast, being the result of physical and botanical researches continued for two years, accompanied by a *vegeto-geographical* map of that country. This Report is now going through the press, and will appear in the 'Mittheilungen' of the Geographical Society of Vienna. A Report on the flora of the same country is also in progress.

During the last five years Professor Lorenz has been engaged in researches on the Liburnian (Croato-Adriatic) coasts, and also in the hitherto unexplored waters of the Quarnero (or Gulf of Fiume), his object being to inquire into the distribution of submarine algæ and animals, on the same principles that had guided the founders of this still youthful department of science, such as CErsted in the Baltic, Forbes in the Ægean and British seas, Sars, Arbjørnsen, Löven in the Scandinavian sea. Professor Lorenz considers that he has made improvements in the method of carrying on researches of this kind. The physical hydrography of the submarine stations in the Quarnero, the geology and quality of the sea-bottom, temperature of the air, density and chemical composition of the sea-water, the currents,

tides, transparency, colour, luminosity, mean temperature at different depths, &c., were methodically observed during four years. Algae and animals of every kind were arranged according to their vertical and horizontal distribution, and their natural groups and paragenesis were deduced from the above-mentioned physico-marine observations. A bathygraphical map, and numerous tables and figures, accompany the work, which in a few weeks will be ready for the press. A memoir on new starfishes and polyps collected and described by the author, intended as a precursor to the larger work, appeared in the 39th volume of the 'Sitzungsberichte' of the Vienna Academy.

I may add, that Professor Lorenz expresses his readiness to procure for the Royal Society any information that may be desired respecting the natural history of the Adriatic coast, adjacent to Fiume, a service which he appears peculiarly well qualified to render by the nature of his previous researches, as well as by his position as Professor of Natural History at Trieste.

I am indebted to Dr. Namias, the Secretary of the I. R. Istituto Veneto, for the titles of memoirs that will appear in the 5th volume of the 'Atti' of that Society. Among these are a memoir on the purple of the Ancients, and the means used in extracting it from the *Murex*, by Professor Bizio. On some methods of applying electricity in the cure of disease, by Dr. Namias, who is also author of a memoir on electricity, in which he has established some new principles, and for which a prize was awarded by the Istituto Milano. A memoir on the hypothesis of the metamorphosis of power, and on the conservation of force, by Professor Turazza, is to appear in vol. ix. of the 'Raccolta,' in 4to. A general theorem on the teeth of bevel-wheels, by Professor Minich. On instantaneous movement about a point, by Professor Bellavitis. Comparative examination of some new species of Lichens, and on *Chrysothrix noli tangere*, by Professor Massalongo.

M. Senoner has supplied information respecting works on the Natural Sciences, published by various Societies and individuals. In Vienna, the 'k. k. Zoologische-botanische Gesellschaft' publishes valuable Transactions. The 'Wiener entomologische Zeitschrift,' conducted by Lederer and Miller, is a periodical of great merit. The 'Oesterr. botanische Zeitschrift,' edited by Dr. Skofitz, contains many excellent papers on the Flora of the Austrian Empire.

The Geological Surveys by the 'Geogn. montanistischer Verein' of

Gratz, are carried on diligently under the direction of M. v. Zollikofer. Yearly volumes are published by the Museum in Linz, and by the Natural History Museum of Klagenfurt. In Trieste, Professor Stoffich is a most assiduous collector of all the productions of the Adriatic. Professor Danilo of Zara, and Professor Lanza of Spalatro, are at work upon a Fauna of the Adriatic. The 'Werner Verein' of Brünn has completed its Geological Surveys, and is proceeding to publish a geological map of Moravia and Silesia. Natural History Museums, for the purpose of making Collections only, have been established in Bregenz, Trent, and Roveredo.

In Milan, the 'Società Italiana di Scienze Naturali' (formerly the Geological Society) publishes Transactions containing excellent papers in every department of the Natural Sciences. The 'Atti' of the Ateneo Italiano (formerly the Accademia Medico-fisico-statistica) contain also many articles on Natural History. Stoppani's Paleontology of Lombardy has reached its 14th number. In Brescia, a beautiful work on fungi (*Miceti dell' agro Bresciana*), in which some new species have been established, has been very lately published by Antonio Venturi.

Professor Zeithammer of Pesth, author of papers in Petermann's 'Mittheilungen' on barometric measurements of heights, and the temperature of springs in the mountainous districts of Agram and Sambor, and on the mode of writing Sclavonian names of places in other languages, is about to publish in the same journal an essay on the natural grouping of the mountain ranges of Croatia, giving the elevations of the principal stations above the sea-level, to be followed by a separate work on the hypsometry of Sclavonia, and the military frontier.

With the view of exhibiting to the eye the ethnographic relations of separate tribes, he has just published in the 'Mittheilungen der k. k. geographischen Gesellschaft,' ideas on the establishment of an Austrian Ethnographic Museum, which, in its principal features, would be equally applicable to any other country.

M. Steinheil has communicated to the Munich Academy of Sciences a simple method of adjusting an equatorial, by levelling the declination axis and the finder, and observing transits of stars with the declination axis horizontal. He also exhibited an 'aplanatic' object-glass, constructed on the principle indicated by Gauss in the first volume of

Bohnenberger's 'Zeitschrift für Astronomie,' having an aperture of 3 inches, and a focal length of 46 inches, French measure, which bears a power of from 300 to 360 extremely well. The success of this object-glass has induced him to attempt the construction of a similar refractor with an aperture of 54 lines, and a focal length of only 48 inches.

Professor v. Kobell has announced to the same Academy the discovery of a new acid, dianic acid, in the mineral called tantalite, from Tammela, euxenite, æschynite, and samarskite.

*December 20, 1860.*

General SABINE, Treasurer and Vice-President, in the Chair.

The following communications were read :—

- I. "Notice of recent Scientific Researches carried on abroad."  
By the FOREIGN SECRETARY.

On the 29th of last November, an extract was read before the Berlin Academy from a memoir on the magnetization of iron and steel, by Professor Wiedemann. In following up his investigations on the relation between the magnetic and mechanical properties of iron and steel, published in the 'Verhandlungen der Baseler Naturforschenden Gesellschaft,' Band II., Professor Wiedemann has obtained the following results :—

1. If an iron wire be twisted during or even after the passage of a voltaic current through it, the wire becomes magnetic.

When the wire is twisted in the manner of a right-handed screw, the point at which the current enters becomes a south pole, in the opposite case it becomes a north pole. If, during the passage of the current, the wire be twisted in different directions, the polarity changes with the direction of the twist. If it be twisted in different directions after the interruption of the current, the magnetism produced by the first twisting diminishes rapidly.

2. If a voltaic current be transmitted through a magnet in the direction of its axis, the magnet will twist.

The experiments were made with wires of iron and steel, of from 1 to 2 millimetres in diameter, surrounded by a voltaic helix, and stretched



by weights attached to their lower ends. The torsion was measured by observing the image of a scale reflected in a mirror fastened to the lower end of the wire.

When the south pole of the magnetized wire was uppermost, and the current descends, the lower end of the wire, as seen from above, twists in the direction of the motion of the hands of a watch. A change in the direction of either the transmitted or the magnetizing current changes the direction of the torsion.

As long as the magnetizing current remains constant, the torsion increases with the intensity of the transmitted current till it reaches a maximum.

When the intensity of the magnetizing current increases, the transmitted current remaining unchanged, the torsion diminishes.

The torsion of the wire is nearly independent of the weight by which it is stretched, as long as the intensities of the magnetizing and the transmitted current remain invariable.

3. The same torsion is produced when a voltaic current is transmitted through iron wires which have been for some time suspended vertically, and thus magnetized by the magnetism of the earth, or which have acquired permanent magnetism in any other manner.

4. A voltaic current being transmitted through an iron wire suspended in the axis of a spiral, if a weak current be afterwards transmitted through the helix, the wire will twist in the same direction as if the current had been transmitted first through the spiral and afterwards through the wire. The torsion increases with the intensity of the magnetizing current till it attains a maximum. On increasing further the intensity of the magnetizing current, the torsion diminishes.

5. If an iron wire be suspended in the axis of a helix, and temporarily magnetized, the intensity of the temporary magnetism will be weakened on transmitting a voltaic current through the wire.

On interrupting the current through the wire, the temporary magnetism of the wire will be increased. If the circuit be completed repeatedly, whatever be the direction in which the current is transmitted through the wire, the temporary magnetism will diminish, and on interrupting the current will resume its original intensity.

6. If a voltaic current be transmitted through the iron wire after the magnetizing current has been interrupted, its permanent magnetism will be weakened,

On interrupting the current the permanent magnetism will increase a little, but will not regain its original intensity. Repeated transmission of a current through the wire produces only a feeble diminution of its permanent magnetism. If a current be transmitted through the wire in an opposite direction, the magnetism will again be greatly enfeebled.

7. When a large portion of the permanent magnetism has been destroyed by repeated inversion of the current transmitted through the wire, and a current is made to pass through it first in one direction and then in the contrary direction, the magnetization in one case is found to be much stronger than in the other. At the same time a very gradual diminution of magnetism is continually observable.

In the opinion of Professor Wiedemann, these phenomena may be explained by the self-same hypotheses which he used to illustrate his former observations on the relation between torsion and magnetism. These are:—

1. The hypothesis of molecular magnets, which take a direction under the influence of a voltaic current and are thereby caused to rotate round their centres of gravity, and also to glide past one another, as in the case of torsion.

2. The hypothesis, that the molecular magnets, after their movements of translation and rotation, do not return quite to their original unmagnetic positions of equilibrium, even after the disturbing forces have ceased to act, so that bodies composed of these molecular magnets retain a permanent magnetism, and in certain cases, a permanent change of form, viz. torsion.

## II. "Preliminary Notice of Researches into the Chemical Constitution of Narcotine and of its Products of Decomposition."

By A. MATTHIJSSEN, Esq., and GEORGE C. FOSTER, Esq.

Communicated by Professor A. W. WILLIAMSON, Ph.D.

Received October 29, 1860.

### I. *Composition of Narcotine.*

The announcement made by Wertheim\* and Hinterberger† of the probable existence of various kinds of narcotine, rendered it necessary

\* Liebig and Kopp's *Jahresbericht*, 1851, p. 469.

† *Ibid.*

to commence the present investigation by a series of analyses of our material, in order to ascertain which variety of narcotine we were dealing with.

The narcotine employed was obtained from Mr. Morson, to whom we are greatly indebted for the scrupulous care bestowed on its preparation and purification. He stated that it was extracted from the residues which had accumulated during the preparation of very large quantities of morphine and codeine, from opium of various qualities and from various sources. If, therefore, distinct varieties of narcotine exist, there was reason to expect that our narcotine would prove to be a mixture of several of them. The results of all our analyses, however, agree with the formula  $C^{22}H^{23}NO^7$ , as shown by the following Table, which gives the highest, lowest, and mean results obtained :—

	Calculated.		Found.		
			Maxima.	Minima.	Mean.
$C^{22}$ .....	264	63·92	64·00	63·42	63·79
$H^{23}$ .....	23	5·57	6·05	5·69	5·81
N .....	14	3·39	3·40	3·26	3·32
$O^7$ .....	112	27·12	27·53	26·72	27·08
$C^{22}H^{23}NO^7$	413	100·00			

The formula which has been generally admitted since the publication of Wöhler's\* and Blyth's† researches on narcotine, namely,  $C^{22}H^{23}NO^7$ , requires the following per-centages :—

Carbon .....	64·61
Hydrogen .....	5·85
Nitrogen .....	3·30
Oxygen .....	26·24

We may here remark that the recorded analyses of narcotine and its salts, with the exception of one by Dr. Hofmann, published by Blyth, agree at least as well with the former as with the latter formula ; moreover, during the course of experiments made with several pounds of narcotine, we have observed nothing, either in the behaviour of this base itself, or in the nature or proportions of its pro-

\* Ann. Chem. Pharm. vol. i. p. 1.

† Ibid. p. 29 ; Mem. Chem. Soc. vol. ii. p. 163.

ducts of decomposition, to indicate that it was variable in composition. Further data are, however, needed for the final decision of this question, and we shall accordingly feel very much indebted to any chemist who has a specimen of narcotine of well-ascertained origin, or which he believes to have a different composition from that given above, if he will kindly spare us a sufficient quantity for analysis.

## II. *Composition of Cotarnine.*

The combustion of cotarnine with oxide of copper and oxygen, as well as the determination of the proportion of platinum in its chloroplatinate, leads us to adopt the formula  $C^{12}H^{13}NO^3$  for this base. The formula usually adopted contains one more atom of carbon; but, independently of our analytical results, the supposition that cotarnine contains only twelve atoms of carbon is supported by the simple manner in which the action of oxidizing substances on narcotine can then be expressed, namely, by the equation



and, as will be shown hereafter, by the manner in which cotarnine is decomposed by dilute nitric acid.

## III. *Decompositions of Opianic Acid.*

Opianic acid is readily decomposed when heated with strong hydriodic acid; no iodine is set free, but iodide of methyl is formed in considerable quantity at the same time as a non-volatile substance, very easily altered by heat and exposure to air, especially if in contact with alkali, the precise nature of which we have not yet been able to ascertain.

When opianic acid is heated with an excess of a very strong solution of potash, it splits up into meconin and hemipinic acid. These substances were found by experiment to be formed in proportions corresponding to the equation



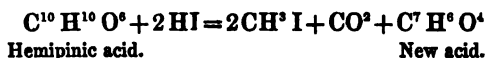
The meconin thus produced has all the characters which have been ascribed by previous observers to meconin obtained by other processes; its identity was further established by analysis, and by the preparation of chloro- and nitro-meconin, the former of which was analysed.

The hemipinic acid was also found to be identical with that obtained directly from narcotine: the acid and its silver-salt were analysed.

Having thus found a method by which meconin and hemipinic acid can be produced with certainty and in large quantities, we intend to make an extended investigation of them and of opianic acid, in the hope of discovering the nature of the relationship of these three bodies to each other and to narcotine. The principal results which we have hitherto obtained in this direction are as follows.

*Action of Hydriodic Acid on Meconin.*—Meconin is decomposed by hydriodic acid like opianic acid, giving iodide of methyl and an easily alterable substance, the nature of which has not been determined.

*Action of Hydriodic Acid on Hemipinic Acid.*—Hemipinic acid, heated with concentrated hydriodic acid to within a few degrees of the boiling-point of the latter substance, is decomposed into iodide of methyl, carbonic acid, and an acid of the formula  $C^7 H^6 O^4$ . It was found by direct experiment that two atoms of iodide of methyl are formed from each atom of hemipinic acid, so that the following equation probably represents the reaction:—



The new acid is moderately soluble in cold water, and very soluble in boiling water, alcohol, and ether; its solution has a strongly acid reaction with test-paper. It separates from hot water in small needle-shaped crystals containing 14·80 per cent. water of crystallization, which they lose at 100° (the formula  $C^7 H^6 O^4 + 1\frac{1}{2} H^2 O$  corresponds to 14·92 per cent. water); at a higher temperature the acid melts and sublimes without apparent alteration.

Dried at 100°, it gave the following results on analysis:—

	Calculated.		Found. (mean)
$C^7$ .....	84	54·55	54·38
$H^6$ .....	6	3·89	3·91
$O^4$ .....	64	41·56	41·71
	<hr/> 154	<hr/> 100·00	<hr/> 100·00

When the dry acid is heated in the air to a little above 100°, it

slowly oxidizes and becomes brown; the same change takes place more rapidly when a solution of it, especially if neutral or alkaline, is evaporated. A solution of the acid immediately reduces ammonio-nitrate of silver, even in the cold; with sulphate of copper and a slight excess of potash it gives a yellowish-green solution, from which suboxide of copper is precipitated on warming. The free acid, or its ammonia-salt, gives a very intense blue coloration with perchloride of iron. The colour thus produced is changed to blood-red (exactly resembling the red produced by the sulphocyanates) by ammonia, and is destroyed by strong acids, being restored by dilution with water, or by neutralization by an alkali: like the colouring matter obtained by Anderson by the action of sulphuric acid on opianic acid, it is entirely removed from solution by alumina.

We have not yet obtained any of the salts of the new acid in a state fit for analysis, and prefer not to propose a name for it until its relationship to other bodies has been more thoroughly examined; its formula, however, assigns to it a place in the following series—

$C^7 H^6 O$ .....	Oil of bitter almonds.
$C^7 H^6 O^2$ .....	Benzoic acid.
$C^7 H^6 O^3$ .....	Salicylic acid.
$C^7 H^6 O^4$ .....	New acid.
$C^7 H^6 O^5$ .....	Gallic acid.
$C^7 H^6 O^6$ .....	Tannoylic acid (?).

It is remarkable that salicylic and gallic acids both give colorations with perchloride of iron much resembling that produced by the acid  $C^7 H^6 O^4$ .

#### IV. *Action of dilute Nitric Acid on Cotarnine.*

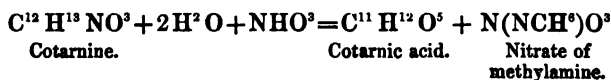
By gently heating cotarnine with very dilute nitric acid, we have obtained nitrate of methylamine and a new acid, *cotarnic acid*, but have not hitherto found out the conditions necessary for the certain production of the latter substance.

Cotarnic acid dissolves easily in water, giving a solution which reacts strongly acid with litmus-paper; it dissolves only sparingly in alcohol, and is precipitated from its alcoholic solution by ether. Heated with an excess of sodium, it gives no trace of cyanide, and therefore contains no nitrogen. With perchloride of iron it gives no coloration; with acetate of lead it gives a white precipitate insoluble in ex-

cess of acetate; with nitrate of silver it gives a precipitate which is very slightly soluble in hot water. The silver-salt, crystallized from water, was found to contain  $C^{11}H^{10}Ag^2O^5$ : on analysis it gave the following results:—

	Calculated.		Found (mean).
$C^{11}$ .....	132	30·14	29·67
$H^{10}$ .....	10	2·27	2·17
$Ag^2$ .....	216	49·32	49·24
$O^5$ .....	80	18·27	18·92
$C^{11}H^{10}Ag^2O^5$	438	100·00	100·00

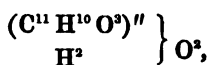
The formation of cotarnic acid is therefore represented by the equation



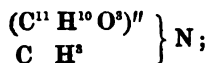
It is possible that the substance obtained by Anderson by the action of nitric acid on narcotine (Chem. Soc. Quart. Journ. vol. v. p. 265; Gerhardt, 'Traité,' vol. iv. p. 80), and supposed by him to be hydrate of meconin (*Opianyl*, Anderson), may have been cotarnic acid, with the composition of which Anderson's analyses closely agree, as shown by the following comparison:—

	Calculated.		Anderson.	
$C^{11}$ .....	132	58·93	58·83	58·84
$H^{12}$ .....	12	5·36	5·17	5·42
$O^5$ .....	80	35·71	36·00	35·74
	224	100·00	100·00	100·00

If cotarnic acid be represented by the formula



cotarnine becomes methyl-cotarnimide—



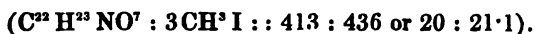
if, however, we retain the formula  $C^{12}H^{13}NO^3$  for cotarnine, no simple relation is apparent between it and cotarnic acid.

V. *Conclusion.*

In the absence of more definite knowledge of the constitution of meconin and opianic and hemipinic acids, it is obviously useless to try to assign a rational formula to narcotine. According to the formulæ which we have adopted for narcotine and cotarnine, narcotine contains the elements of cotarnine and meconin :—



It will be seen that these formulæ are the same as those of the methyl-narcotine and methyl-cotarnine of Hinterberger and Wertheim. The ground upon which Wertheim admitted the existence of ethyl- and propyl-narcotine was the formation of volatile bases containing  $\text{C}^2 \text{H}^7 \text{N}$  and  $\text{C}^3 \text{H}^9 \text{N}$  by the distillation of narcotine with potash. An experiment which we have made goes some way towards explaining the formation of these bases without assuming the existence of more than one variety of narcotine. Having so frequently observed the formation of methyl-compounds from the derivatives of narcotine, we tried the direct action of hydriodic acid on this base, expecting to obtain iodide of methyl. By distilling 20 grms. of narcotine with concentrated hydriodic acid, 19 grms. of pure iodide of methyl were obtained, a quantity which corresponds, as nearly as could be expected, with three atoms of iodide of methyl for one atom of narcotine\*,



Narcotine therefore contains three atoms of methyl so combined as to be easily separable†; and it is very probable that when it is distilled with potash, according to the conditions of the experiment, sometimes nearly pure ammonia is evolved, while, at other times, methylamine,  $\text{CH}^3 \text{N}$ , dimethylamine,  $\text{C}^2 \text{H}^7 \text{N}$ , or trimethylamine,  $\text{C}^3 \text{H}^9 \text{N}$ , predominates.

We wish not to close without acknowledging our obligation to Dr. M. Holzmann for very valuable assistance rendered to us at the commencement of our investigation.

\* It is possible that narcotine will prove to be an economical, as it is certainly the most convenient, source of iodide of methyl.

† Gerhardt (*Traité*, iv. 64) had previously observed the production of a volatile substance, which he supposed to be nitrate of ethyl or of methyl, by the action of nitric acid on narcotine.



III. "Researches on the Arsenic-Bases." By A. W. HORMANN, LL.D., F.R.S. Received November 24, 1860.

In a previous note\* I have shown the existence of a group of diatomic bases, containing phosphorus and arsenic, which are formed by the action of monarsines on the bromethylated bromide, so frequently mentioned in my researches on the phosphorus-bases. The idea naturally suggested itself to examine the deportment of this salt under the influence of monostibines, with the view of producing the phospho-stibonium-compounds. The two bodies react upon one another, but only after protracted digestion or exposure to rather high temperatures. The product of the reaction is complex, yielding a comparatively small quantity of a difficultly soluble platinum-salt of diatomic appearance. I have repeatedly modified the circumstances and analysed the products in the form of platinum-salts; I omit to quote the detail of these experiments, since they have failed to disentangle the difficulties of the reaction.

Some experiments upon the deportment of dibromide of ethylene with triethylarsine were more successful. The reaction between these two bodies had been selected as a subject of inquiry by Mr. W. Valentin, to whom I am indebted for valuable assistance at the earlier stage of these researches. Circumstances have subsequently prevented Mr. Valentin from carrying out his plan, and I have therefore to take upon myself the responsibility for the following statements.

*Action of Dibromide of Ethylene upon Triethylarsine.*

MONARSONIUM SERIES.

The experience gathered during the examination of the phosphorus-bodies, enabled me to establish the nature of this reaction by a comparatively small number of platinum-determinations.

*Bromide of Bromethyl-triethylarsonium.*—To avoid as far as possible the formation of the second product, a mixture of triethylarsine with a very large excess of dibromide of ethylene was digested in sealed tubes at a temperature not exceeding 50° C. Notwithstanding the low temperature, the tubes invariably contained compressed gases; the product of the reaction was treated with water, which extracted

\* Proceedings of the Royal Society, "Researches on the Phosphorus-Bases. No. IX. Phosphammonium Compounds," vol. x. p. 608.

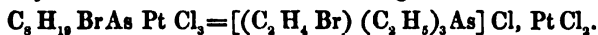
a soluble bromide from the ethylene-compound unacted upon. On evaporation, a beautiful bromide was left, which being copiously soluble in boiling, and sparingly soluble in cold alcohol, could be readily recrystallized from absolute, and even from common alcohol. In water this substance is excessively soluble, and therefore scarcely crystallizable from an aqueous solution.

Analysis, as might have been expected, proved this salt to be the analogue of the bromethylated triethylphosphonium-salt. It contains



The bromide of bromethyl-triethylarsonium, the composition of which is sufficiently established by the analysis of the corresponding platinum-salt, can be obtained in beautiful crystals. Their form was determined by Quintino Sella; it corresponds exactly with that of the corresponding phosphorus-compound.

*Platinum-salt.*—The solution of the previous salt, converted by treatment with chloride of silver into the corresponding chloride, yields with dichloride of platinum, splendid needles of a double salt, difficultly soluble in cold and even in boiling water, which contain



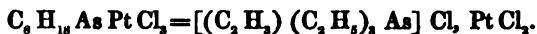
#### *Compounds of Vinyl-triethylarsonium.*

The bromide of bromethyl-triethylarsonium, like the corresponding phosphorus-compound, loses its latent bromine under the influence of oxide of silver. If the solution of the bromide be precipitated by an excess of nitrate of silver, one half of the bromine separates as bromide of silver; the clear filtrate mixed with ammonia yields the second half of the bromine in the form of a dense precipitate. Nevertheless the reaction differs from that observed in the phosphorus-series. The bromide of the bromethylated phosphonium, as has been pointed out in a former part of the researches on the phosphorus-bases, is almost invariably converted into an oxethylated body, its transformation into a vinyl-compound being altogether exceptional. The bromide of the bromethylated arsonium, on the other hand, yields as a rule the vinyl-body of the series, the formation of an oxethylated compound taking place only under particular circumstances, in fact so rarely as to leave some doubt regarding the existence of this term of the series.

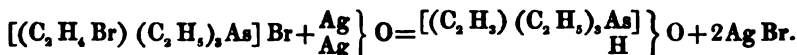
The bromide of bromethylated arsonium, treated with an excess of

\* H = 1; O = 16; S = 32; C = 12.

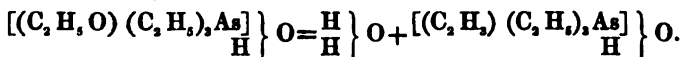
oxide of silver, yields a powerfully alkaline solution, the nature of which was determined by the analysis of the corresponding platinum-salt. Transformed into the chloride and precipitated with dichloride of platinum, this solution yielded beautiful rather soluble octahedra which were found to contain



The analysis of this salt shows that the transformation of the bromethylated compound ensues according to the following equation,



The idea suggested itself that the vinyl-compound obtained in this reaction might be a secondary product resulting from the decomposition of an oxethylated compound of limited stability formed in the first instance,



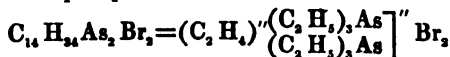
It was with the view of avoiding this decomposition that in one of the operations the digestion was accomplished at the common temperature; the result, however, showed that even in this case the vinyl-compound was obtained.

Nevertheless the oxethylated body appears to exist: under circumstances which were not sufficiently well observed at the time, the action of oxide of silver upon bromide of bromethylated triethylarsonium yielded an octahedral platinum-salt, which on analysis furnished exactly the platinum-percentage of the oxethylated compound.

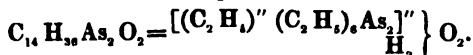
#### DIARSONIUM SERIES.

##### *Dibromide of Ethylene-hexethyldiarsonium.*

The bromide or chloride of the bromethylated arsonium-compound is but slowly acted upon by triethylarsine at 100° C. Two days' digestion at that temperature had produced but a slight impression; at 150° the reaction is accomplished in two hours. The phenomena now to be recorded presented themselves in the succession repeatedly observed in the diphosphonium-series. The dibromide



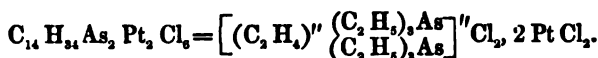
yielded, when debromized, the powerful alkali



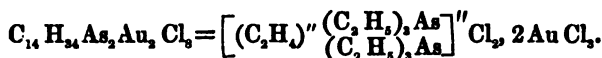
Treated with acids, this alkali produces a series of fine salts, amongst which the di-iodide deserves to be mentioned; it equals in beauty the corresponding diphosphonium-compound.

I have fixed the composition of the series by the analysis of the platinum-salt and gold-salt.

*Platinum-salt.*—Pale-yellow crystalline precipitate, similar to the diphosphonium-compound, difficultly soluble in water, soluble in boiling concentrated hydrochloric acid, from which it crystallizes on cooling. It contains



*Gold-salt.*—The dichloride obtained after separating the platinum in the previous analysis by sulphuretted hydrogen, was precipitated by trichloride of gold; yellow slightly crystalline precipitate, soluble in hydrochloric acid, from which it crystallizes in golden-coloured plates. The formula of this salt is



#### ARSAMMONIUM SERIES.

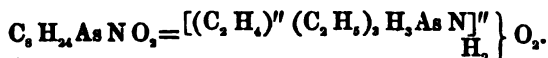
Bromide of bromethylated triethylarsonium, as might have been expected, is capable of fixing ammonia and monamines, giving rise to the formation of a group of compounds not less numerous than the bodies mentioned in the phosphorus-series. I have been satisfied to study the action of ammonia upon the bromide.

#### *Dibromide of Ethylene-triethylarsammonium.*

Reaction complete in two hours at 100°. The product contains the dibromide,

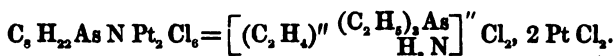


this salt is converted by oxide of silver into the stable caustic base

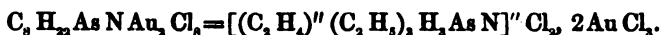


the composition of which was determined by the analysis of the platinum-salt and gold-salt.

*Platinum-salt.*—Needles, difficultly soluble in boiling water, soluble in concentrated hydrochloric acid, from which well-formed crystals are deposited, containing



*Gold-salt.*—Yellow compound precipitated from the dichloride obtained in the previous platinum-determination, on addition of tri-chloride of gold, soluble in hydrochloric acid, deposited from this solution in golden-yellow plates of the composition

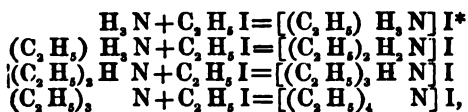


I have also made a few experiments on the action of dibromide of ethylene upon triethylstibine. The reaction is slow, and requires long-continued digestion at temperatures higher than that of boiling water. The tubes invariably contained much gas; and the product of the reaction proved to be a complex mixture of several compounds, many of them secondary, which in no way invited me to a more minute examination of this process. I omit to quote the few platinum- and chlorine-determinations which were made, since they do not admit of a simple interpretation.

#### IV. "Contributions towards the History of the Monamines."—

No. IV. Separation of the Ethyl-Bases. By A. W. HOFMANN, LL.D., F.R.S. Received November 28, 1860.

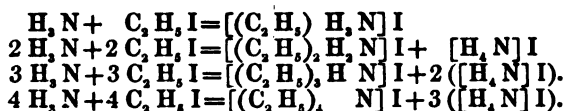
The preparation of the ethyl-bases by the action of ammonia upon iodide of ethyl, presents a difficulty which greatly interferes with the general application of this otherwise so convenient method. This difficulty consists in the simultaneous formation of all the four ethyl-bases. The equations



are an ideal representation of the four different phases through which ammonia passes during its transformation into iodide of tetrethyl-ammonium. In practice it is found impossible to carry out this transformation in the several steps indicated by these equations. The first substitution-product, generated as it is in the presence of the agent of substitution, is immediately acted upon again, the second

\* H=1; O=16; C=12, &c.

product being formed, which in its turn may be converted into the third and even into the fourth compound. The following equations represent perhaps more correctly the final result of the several changes which are accomplished in the reaction of ammonia on iodide of ethyl.



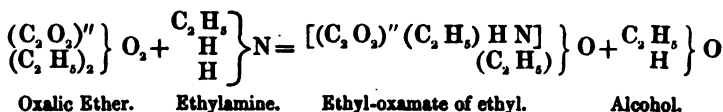
The mixture of iodides, when submitted to the action of potassa, yields ammonia, ethylamine, diethylamine, and triethylamine, the hydrate of tetrethylammonium, which is liberated, splitting into ethylene, triethylamine, and water. The separation of the three ethyl-ammonias presents unusual difficulties. The differences between their boiling-points being rather considerable,

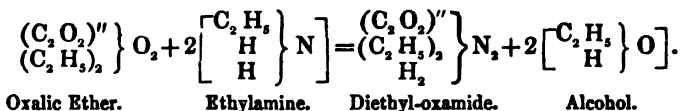
Ethylamine, boiling-point	..... 18°
Diethylamine, „ „	..... 57°·5
Triethylamine, „ „	..... 91°

it was thought that they might be readily separated by distillation. Experiments made with very large quantities showed, however, that even after ten fractional distillations the bases were far from being pure.

After many unsuccessful attempts, I have found a simple and elegant process by which the three ethyl-bases may be easily and perfectly separated. This process consists in submitting the anhydrous mixture of the three bases to the action of anhydrous oxalate of ethyl. By this treatment, ethylamine is converted into *diethyl-oxamide*, a beautifully crystalline body very difficultly soluble in water, diethylamine into *ethyl-oxamate of ethyl*, a liquid boiling at a very high temperature, whilst triethylamine is not affected by oxalic ether

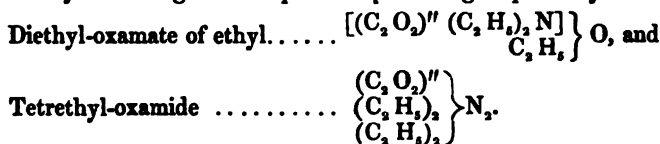
By the action of oxalic ether upon ethylamine, two substances may be formed, viz. *ethyl-oxamate of ethyl* and *diethyl-oxamide*.





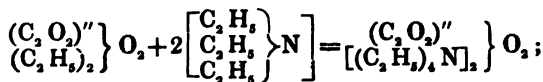
In practice it appears that the *second* of these compounds only is produced.

In the action of oxalate of ethyl upon diethylamine, two similar phases may be distinguished capable of producing respectively



In practice the *first* of these two compounds only is generated.

The action of oxalate of ethyl upon triethylamine might have involved the formation of the secondary oxalate of tetrethylammonium,



under the circumstances under which I have worked, the two substances do not combine.

The product of the reaction of oxalate of ethyl upon the mixture of the ethyl-bases, when distilled in the water-bath, yields *triethylamine free from ethylamine and diethylamine*.

The residue in the retort solidifies on cooling into a fibrous mass of crystals of diethyloxamide, which are soaked with an oily liquid. They are drained from the oil and recrystallized from boiling water. Distilled with potassa, these crystals furnish *ethylamine free from diethylamine and triethylamine*.

The oily liquid is cooled to 0°, when a few more of the crystals are deposited; it is then submitted to distillation. The boiling-point rapidly rises to 260°. What distils at that temperature is pure diethyl-oxamate of ethyl, from which, by distillation with potassa, *diethylamine free from ethylamine and triethylamine* may be obtained.

The Society then adjourned to Thursday, January 10, 1861.

January 10, 1861.

Major-General SABINE, R.A., Treasurer and Vice-President,  
in the Chair.

The Right Hon. Sir William Erle was admitted into the Society.

The following communications were read:—

- I. "On the Equation for the Product of the Differences of all but one of the Roots of a given Equation." By ARTHUR CAYLEY, Esq., F.R.S. Received November 30, 1860.

(Abstract.)

It is easy to see that for an equation of the order  $n$ , the product of the differences of all but one of the roots will be determined by an equation of the order  $n$ , the coefficients of which are alternately rational functions of the coefficients of the original equation, and rational functions multiplied by the square root of the discriminant. In fact, if the equation be  $\phi v = (a, \dots, \chi v, 1)^n = a(v-a)(v-\beta) \dots$ , then putting for the moment  $a=1$ , and disregarding numerical factors,  $\sqrt{\square}$ , the square root of the discriminant, is equal to the product of the differences of the roots, and  $\phi'a$  is equal to  $(\alpha-\beta)(\alpha-\gamma) \dots$ , consequently the product of the differences of the roots, all but  $\alpha$  is equal to  $\sqrt{\square} \div \phi'a$ , and the expression  $\frac{1}{\phi'a}$  is the root of an equation of the order  $n$ , the coefficients of which are rational functions of the coefficients of the original equation. I propose in the present memoir to determine the equation in question for equations of the orders three, four, and five: the process employed is similar to that in my memoir "On the Equation of Differences for an equation of any Order, and in particular for Equations of the Orders Two, Three, Four, and Five," Phil. Trans. t. cl. p. 112 (1860), viz. the last coefficient of the given equation is put equal to zero, so that the given equation breaks up into  $v=0$  and into an equation of the order  $n-1$  called the reduced equation; and this being so, the required equation breaks up into an equation of the order  $n-1$  (which, however, is not, as for the equation of differences, that which corresponds to the reduced equation) and into a linear equation; the



equation of the order  $\overline{n-1}$  is calculated by the method of symmetric functions ; and combining it with the linear equation, which is known, we have the required equation, except as regards the terms involving the last coefficient, which terms are found by the consideration that the coefficients of the required equation are seminvariants. The solution leads immediately to that of a more general question ; for if the product of the differences of all the roots except  $\alpha$ , of the given equation  $\phi v = (*\chi v, 1)^n = a(v-\alpha)(v-\beta) \dots = 0$  (which product is a function of the degree  $n-2$  in regard to each of the roots  $\beta \gamma \delta \dots$ ), is multiplied by  $(x-\alpha y)^{n-2}$ , the function so obtained will be the root of an equation of the order  $n$ , the coefficients of which are covariants of the quantic  $(*\chi x, y)^n$ , and these coefficients can be at once obtained by writing, in the place of the seminvariants of the former result, the covariants to which they respectively belong. In the case of the quintic equation, one of these covariants is, in regard to the coefficients, of the degree 6, which exceeds the limit of the tabulated covariants, the covariant in question has therefore to be now first calculated. The covariant equations for the cubic and the quartic might be deduced from the formulæ Nos. 119 and 142 of my Fifth memoir on Quantics, Phil. Trans. t. cxlviii. pp. 415-427 (1858) ; they are in fact the bases of the methods which are there given for the solution of the cubic and the quartic equations respectively ; and it was in this way that I was led to consider the problem which is here treated of.

## II. "Description of a new Optical Instrument called the 'Stereotrope.'" By WILLIAM THOMAS SHAW, Esq. Communicated by WARREN DE LA RUE, Esq. Received Dec. 6, 1860.

This instrument is an application of the principle of the stereoscope to that class of instruments variously termed thaumatropes, phantascopes, phenakistoscopes, &c., which depend for their results on "persistence of vision." In these instruments, as is well known, an object represented on a revolving disc, in the successive positions it assumes in performing a given evolution, is seen to execute the movement so delineated ; in the stereotrope the effect of solidity is superadded, so that the object is perceived as if in motion and with

an appearance of relief as in nature. The following is the manner in which I adapt to this purpose the refracting form of the stereoscope.

Having procured eight stereoscopic pictures of an object—of a steam-engine for example—in the successive positions it assumes in completing a revolution, I affix them, in the order in which they were taken, to an octagonal drum, which revolves on a horizontal axis beneath an ordinary lenticular stereoscope and brings them one after another into view. Immediately beneath the lenses, and with its axis situated half an inch from the plane of sight, is fixed a solid cylinder, 4 inches in diameter, capable of being moved freely on its axis. This cylinder, which is called the eye-cylinder, is pierced throughout its entire length (if we except a diaphragm in the centre inserted for obvious reasons) by two apertures, of such a shape, and so situated relatively to each other, that a transverse section of the cylinder shows them as cones, with their apices pointing in opposite directions, and with their axes parallel to, and distant half an inch from, the diameter of the cylinder. Attached to the axis of the eye-cylinder is a pulley, exactly one-fourth the size of a similar pulley affixed to the axis of the picture-drum, with which it is connected by means of an endless band. The eye-cylinder thus making four revolutions to one of the picture-drum, it is evident that the axes of its apertures will respectively coincide with the plane of sight four times in one complete revolution of the instrument, and that, consequently, vision will be permitted eight times, or once for each picture.

The cylinder is so placed that at the time of vision the *large* ends of the apertures are next the eyes, the effect of which is that when the *small* ends pass the eyes, the axes of the apertures, by reason of their eccentricity, do not coincide with the plane of sight, and vision is therefore impossible. If, however, the position of the cylinder be reversed end for end, vision will be possible only when the *small* ends are next the eyes, and the angle of the aperture will be found to subtend exactly the pencil of rays coming from a picture, which is so placed as to be bisected at right angles by the plane of sight. Hence it follows that, the former arrangement of the cylinder being reverted to, the observer looking along the upper side of the aperture will see a narrow strip extending along the top of the picture; then, moving the cylinder on and looking along the lower side of the

aperture, he will see a similar strip at the bottom of the picture ; consequently, in the intermediate positions of the aperture, the other parts of the picture will have been projected on the retinae. The width of these strips is determined by that of the small ends of the apertures, which measure  $\cdot 125$  inch ; and the diameter of the large ends is  $1\cdot 5$  inch, the lenses being distant 9 inches from the pictures. The picture-drum being caused to revolve with the requisite rapidity, the observer will see the steam-engine constantly before him, its position remaining unchanged in respect of space, but its parts will appear to be in motion, and in solid relief, as in the veritable object. The stationary appearance of the pictures, notwithstanding the fact of their being in rapid motion, is brought about by causing their corresponding parts to be seen, respectively, *only* in the same part of space, and *that* for so short a time that while in view they make no sensible progression. As, however, there is an actual progression during the instant of vision, it is needful to take that fact into account—in order that it may be reduced as far as practicable—in regulating the diameter of the eye-cylinder, and of the apertures at their small ends ; and the following are the numerical data involved in the construction of an instrument with the relative proportions given above :—

The circumference of picture-drum =  $22\cdot 5$  inches (A).

The circumference of eye-cylinder = 12 inches  $\times$  4 revolutions = 48 inches (B).

The diameter of apertures at large ends =  $1\cdot 5$  inch (C).

The diameter of apertures at small ends =  $\cdot 125$  inch (D).

While the large end is passing the eye, the picture under view progresses  $\frac{1\cdot 5}{48} \frac{(C)}{(B)}$  of  $22\cdot 5$  (A), or  $\cdot 703$  inch.

This amount of progression ( $\cdot 703$  in.), if perceived at one and the same instant, would be utterly destructive of all distinctness of definition ; but it is evident that the total movement brought under visual observation at any one moment is  $\frac{\cdot 125}{1\cdot 5} \frac{(D)}{(C)}$  of  $\cdot 703$  inch, or  $\cdot 058$  inch. This movement must necessarily occasion a corresponding slurring, so to speak, of the images on the retina ; and the fact of such slurring not affecting, to an appreciable extent, the distinctness of definition, seems to be referable to a faculty which the mind has

of correcting or disregarding certain discrepant appearances or irregularities in the organ of vision ; as a further illustration of which I may cite the fact, mentioned by Mr. Warren De la Rue in his "Report on Celestial Photography," that the retinal image of a star is, at least under some atmospheric conditions, made up of "a great number of undulating points," which, however, the mind rightly interprets as the effect of the presence before the eye of a single minute object. That this corrective power is, as might be supposed, very limited, may be proved experimentally by this instrument ; for if the small ends be enlarged in only a slight degree, so as to increase this slurring on the retinae, a very marked diminution in clearness of definition is the immediate result.

That form of the stereotrope, in which Professor Wheatstone's reflecting stereoscope is made use of, and which is better adapted for the exhibition of movements that are not only local but progressive in space, it is needless to describe here, because the principles it involves are essentially the same as those which are stated above.

### III. "On the Lunar-Diurnal Variation of the Magnetic Declination obtained from the Kew Photograms\* in the years 1858, 1859, and 1860." By Major-General EDWARD SABINE, R.A., Treas. and V.P.R.S. Received December 19, 1860†.

Having communicated to the Royal Society in a recent paper an analysis of the *disturbances* of the declination in the years 1858 and 1859, shown by the photograms of the Kew Observatory, I propose in the present paper to submit the results of the *lunar-diurnal variation* of the declination in the years 1858, 1859, and 1860, obtained from the same source. The directions of the declination magnet at the instant of the commencement of every solar-hour having been tabulated from the photograms, and the final normals

\* The term Photogram is adopted in place of Photograph in conformity with modern usage.

† [Note added on February 8th, 1861.] When this communication was read to the Royal Society on January 10th, 1861, it contained the lunar-diurnal variation for the years 1858 and 1859 only : whilst it was passing through the press, the calculation of the lunar-diurnal variation for 1860 was completed, and the results in that year have been added.

for each month and hour computed, after the omission from the record of all the hourly directions which deviated  $3'3$  from their final normals,—the *differences* were taken between each of the remaining hourly directions and the final normal of the same month and hour, and were entered afresh in *lunar* monthly tables, having the lunar days in successive horizontal lines, and the twenty-four lunar hours in vertical columns, each difference being placed under the lunar hour to which it most nearly approximated. The entries in these tables should consequently represent directly the lunar influence at the different lunar hours, subject only to minor disturbances; the effects of the solar-diurnal variation as well as of the larger disturbances having been eliminated. The differences were marked with a + sign when the north end of the magnet was east of its mean direction, and with the — sign when west of the same. The differences were then summed up, and hourly, monthly, and annual means taken by the non-commissioned officers of the Royal Artillery employed at Woolwich, under the superintendence of Mr. Magrath.

Having in the former paper exhibited the results of the disturbances at Kew in comparison with those at Hobarton, I propose to do the same with the lunar-diurnal variation treated of in this communication; believing that such comparisons are very conducive to a just appreciation of the systematic character and natural reality of the results, and instructive both by the agreements and disagreements which they exhibit. The lunar-diurnal variation at Hobarton has been obtained for the purpose of this comparison, by a similar process to that which has been described above, from observations at every solar hour during five years (Sundays excepted), from Oct. 1, 1843 to Sept. 30, 1848; omitting as disturbed such observations as deviated  $2'13$  from their respective final normals. The total number of hourly observations was 36,832; the disturbed observations 2606; and the number employed in the lunar-diurnal variation 34,226. As it has been customary to represent such periodical variations by formulæ of well-known character, the results at Kew and Hobarton are here represented by formulæ in which  $\alpha$ , corresponding to  $x$  (the lunar time for which the lunar-diurnal variation is desired), is counted in hours and parts of an hour, multiplied by  $15^\circ$ , from the epoch of the moon's upper culmination. The + sign corresponds (as before)

to a deflection of the north end of the magnet to the east of its mean place, and the — sign to the west.

Kew  $\Delta x = +0''.64 - 2''.54 \sin(a + 6^\circ.2) - 9''.74 \sin(2a + 59^\circ.8)$ .

Hobarton  $\Delta x = -0''.1 + 1''.14 \sin(a + 344^\circ.7) + 6''.8 \sin(2a + 43^\circ.2)$ .

In computing the lunar-diurnal variation by means of these formulæ, the coefficient of the term which includes the sine of twice the hour-angle is of principal importance: the subsequent terms are comparatively of little significance, and are therefore omitted on the present occasion. When all the terms are employed, the original observed values are reproduced.

Table I. exhibits, at Kew, in column 2 the lunar-diurnal variation as actually observed on the mean of the three years, and in column 3, the same computed by the formula. Column 4 is the lunar-diurnal variation at Hobarton on the mean of the five years as observed, and column 5 the same computed by the formula.

TABLE I.—Lunar-diurnal Variation at Kew and Hobarton.

Lunar Hours.	Kew.		Hobarton.		Lunar Hours.
	Observed.	Computed.	Observed.	Computed.	
Col. 1.	Col. 2.	Col. 3.	Col. 4.	Col. 5.	Col. 6.
0	— 6.0	— 8.0	+ 4.8	+ 4.3	0
1	— 11.4	— 10.0	+ 6.1	+ 6.4	1
2	— 8.6	— 9.3	+ 5.2	+ 6.8	2
3	— 5.0	— 6.2	+ 5.9	+ 5.4	3
4	— 3.2	— 1.7	+ 4.2	+ 2.7	4
5	+ 1.4	+ 3.0	0.0	— 0.6	5
6	+ 5.4	+ 6.5	— 4.9	— 3.7	6
7	+ 7.6	+ 8.0	— 6.1	— 5.5	7
8	+ 8.6	+ 7.0	— 4.9	— 5.6	8
9	+ 4.3	+ 3.9	— 3.3	— 4.1	9
10	+ 2.8	— 0.4	— 2.2	— 4.3	10
11	— 3.0	— 4.6	+ 3.6	+ 2.0	11
12	— 10.6	— 7.5	+ 4.9	+ 4.9	12
13	— 10.4	— 8.2	+ 6.6	+ 6.4	13
14	— 7.0	— 6.3	+ 5.9	+ 6.2	14
15	— 2.2	— 2.3	+ 4.1	+ 4.3	15
16	+ 4.8	+ 3.0	+ 1.4	+ 1.1	16
17	+ 10.4	+ 8.0	— 3.4	— 2.7	17
18	+ 13.2	+ 11.6	— 6.4	— 5.9	18
19	+ 12.6	+ 12.7	— 6.5	— 7.7	19
20	+ 7.2	+ 11.1	— 6.6	— 7.8	20
21	+ 6.2	+ 7.1	— 8.4	— 6.0	21
22	— 0.4	+ 1.7	— 1.9	— 2.9	22
23	— 1.4	— 4.6	+ 0.8	+ 0.9	23

The aspect of the lunar-diurnal variation at Kew and Hobarton presents features of great simplicity as well as accord. The form at both stations is a division of the 24 lunar hours into four equal or nearly equal portions, in which the magnet is attracted alternately to the east and to the west of its mean position, which is passed through four times in the progress of the magnet towards two extreme easterly and two extreme westerly deflections: the easterly extremes are about 12 hours apart, and the westerly the same. As far as our present experience goes, this appears to be the general form of the lunar-diurnal variation of the declination at all the stations at which it has been examined; it is also that of the corresponding variations of the Dip and Total force. At Hobarton, where the results are obtained from five years of observation, there is scarcely any difference deserving of notice between the amplitudes of the extremes on either side of the upper culmination and those on either side of the lower culmination. At Kew, where the results are obtained from only three years, the extreme deflections are not quite so symmetrical in amount, but they may become more so as additional years are brought into the account. The amplitude of the oscillation on a mean of the two alternations is  $9''\cdot74$  at Kew and  $6''\cdot8$  at Hobarton, a difference in correspondence with the difference in the opposite direction of the antagonistic retaining force of the earth's magnetism at the two stations, which is  $3\cdot7$  at Kew and  $4\cdot5$  at Hobarton. On inspecting the Table, we see that the lunar times when the moon's influence produces no deflection (or the times when the variation is zero), are four, and are nearly the same at Kew and at Hobarton, two of them being a little more than an hour before the moon's passage of the meridian, both at her upper and lower culminations, and the other two intermediate. So far the two stations are alike; but in regard to the direction towards which the magnet is deflected (if in conformity with general usage we speak in both hemispheres of the north end of the magnet, as is done in the Table), we see that the variation becomes west at Kew when it becomes east at Hobarton, and *vice versa*; the phases, while agreeing in hours at the two stations, having throughout opposite signs.

By extending the comparison of the lunar hours at which the lunar variation passes through its zero-points to other stations than Kew and Hobarton, we are made aware of differences which appear to

deserve particular attention in theoretical respects. At Pekin, for example—which may be advantageously compared with Kew, being both in the same hemisphere, but Pekin some degrees nearer the equator—the variation is zero in the passage of the north end of the magnet from east to west at  $20\frac{1}{2}$  lunar hours, or  $2\frac{1}{2}$  hours earlier than the corresponding epoch at Kew. Again, at the Cape of Good Hope, situated in the same hemisphere with Hobarton, but some degrees nearer the equator, the variation is zero in the passage of the north end of the magnet from west to east also at  $20\frac{1}{2}$  lunar hours, or  $2\frac{1}{2}$  hours earlier than the corresponding epoch at Hobarton. Thus there is an accord of precisely the same kind between Pekin and the Cape of Good Hope that there is between Kew and Hobarton, whilst there is a difference between the two pairs of stations of  $2\frac{1}{2}$  hours in the position of the moon relatively to the meridian at which she ceases to exercise a deflecting influence on the magnet. Again, at St. Helena, which is in the same (geographical) hemisphere as Hobarton and the Cape of Good Hope, but still nearer to the equator than either, the lunar influence is zero in the passage from west to east at  $19\frac{1}{2}$  lunar hours, being one hour earlier than at the Cape, and  $3\frac{1}{2}$  hours earlier than at Hobarton.

Where the whole range of the variation of which we have been treating is so small (not more than a few seconds of arc in each lunar day), it may be desirable to show by the accordance of the independent evidence obtained in single years, the degree of confidence which may be placed in the mean results of several years. This may be seen in the Table on the next page, which contains the separate results in each of the five successive years of observation at Hobarton, as well as their mean.

In this Table the principal features of the variation are seen to be substantially alike in each year. The individual results at the several hours in single years are of course somewhat less regular than in the mean of the five years: such small discrepancies are no doubt in great part due to the lesser disturbances which, being below the separating value of  $2'13$ , have been left in the body of the observations. They slightly disfigure the symmetry of the results in single years, but almost entirely disappear when the mean of several years is taken. In order to appreciate justly and fully the confidence to which the whole investigation is entitled, it must be borne in mind that every single



entry in the Table (exclusive of course of the column which exhibits the mean of the five preceding columns) is derived from a wholly independent body of observations which belong to itself alone, and are not employed in the deduction of any of the other entries.

TABLE II. — Lunar-diurnal Variation at Hobarton in the several years from October 1843 to September 1848; omitting disturbed observations differing 2'13 from their final normals.

Lunar Hours.	Years ending September 30th.					Means.	Lunar Hours.
	1844.	1845.	1846.	1847.	1848.		
0	+ 0.6	+ 7.8	+ 3.6	+ 2.4	+ 9.6	+ 4.8	0
1	+ 6.6	+ 9.0	+ 1.2	+ 0.6	+ 13.2	+ 6.1	1
2	+ 4.8	+ 5.4	+ 5.4	+ 6.0	+ 4.2	+ 5.2	2
3	+ 9.6	+ 7.8	+ 7.8	+ 3.6	+ 0.6	+ 5.9	3
4	+ 4.8	+ 6.6	+ 6.0	+ 3.0	+ 0.6	+ 4.2	4
5	- 3.0	+ 2.4	+ 3.0	- 1.8	- 0.6	0.0	5
6	- 7.8	- 6.0	- 1.8	- 7.8	- 1.2	- 4.9	6
7	- 6.0	- 9.6	- 0.6	- 10.8	- 3.6	- 6.1	7
8	- 4.2	- 8.4	- 1.2	- 7.8	- 3.0	- 4.9	8
9	0.0	- 9.0	- 0.6	- 4.8	- 3.0	- 3.3	9
10	- 2.4	- 4.8	- 1.8	- 0.6	- 6.6	- 3.2	10
11	+ 3.0	+ 0.6	+ 3.0	+ 8.4	+ 3.0	+ 3.6	11
12	+ 7.2	+ 2.4	+ 4.8	+ 7.8	+ 2.4	+ 4.9	12
13	+ 12.0	+ 6.6	+ 6.6	+ 4.8	+ 3.0	+ 6.6	13
14	+ 3.0	+ 8.4	+ 7.2	+ 7.8	+ 3.0	+ 5.9	14
15	+ 7.8	+ 4.8	+ 4.2	+ 3.0	+ 0.6	+ 4.1	15
16	+ 1.8	+ 3.6	+ 0.6	+ 0.6	+ 0.6	+ 1.4	16
17	0.0	- 1.2	- 3.6	- 2.4	- 9.6	- 3.4	17
18	- 6.6	- 6.6	- 5.4	- 6.6	- 6.6	- 6.4	18
19	- 4.8	- 5.4	- 8.4	- 7.2	- 6.6	- 6.5	19
20	- 6.6	- 10.2	- 13.2	- 4.2	- 5.4	- 6.6	20
21	- 9.6	- 12.6	- 10.8	- 4.2	- 4.8	- 8.4	21
22	- 4.2	- 0.6	- 10.8	+ 2.4	+ 3.6	- 1.9	22
23	- 2.4	0.0	- 2.4	+ 0.6	+ 8.4	+ 0.8	23

It may operate as an encouragement to those who have not yet subjected their observations to any process of examination or analysis, to perceive, by this example, how substantially satisfactory are the results which may be obtained from even a single year of hourly observations, after the larger disturbances and the solar-diurnal variation have been eliminated.

I have spoken in a recent paper of an unexceptionable test by which we may satisfy ourselves as to the confidence which may be reposed in a series of observations, whether obtained by the eye or tabulated from instrumental traces. Such a test is furnished when the entries at solar hours are rewritten according to the lunar hours to which they most nearly approximate, and when consequently their

original order and relations are changed and are replaced by others which were wholly unforeseen, so that the observations must necessarily be free from the possibility of having been influenced by any mental bias. When we find the effects of a natural law, represented by such minute values as that of the lunar-diurnal variation, exhibited by the observations of a single year with the degree of symmetry shown in Table II, we may safely conclude that the observations themselves are worthy of the labour bestowed in eliciting their results. In this view the Hobarton observations prove themselves to have been not only a faithful, but also an extremely careful series, highly creditable to Captain Kay, R.N., and to the Naval Officers who with him and their Civil Assistant Mr. Jeffery, maintained for so many years the laborious and monotonous duty of hourly observation.

Table III. exhibits the separate results in each of the three years at Kew, as well as their mean.

TABLE III.—Lunar-diurnal Variation at Kew in the years 1858, 1859, and 1860; omitting disturbed observations differing 3'·3 from their final normals.

Lunar Hours.	Year ending December 31.			Means.	Lunar Hours.
	1858.	1859.	1860.		
0	— 6·0	+ 0·6	— 12·6	— 6·0	0
1	— 14·4	— 7·2	— 12·6	— 11·4	1
2	— 10·8	— 9·6	— 5·4	— 8·6	2
3	— 7·8	— 4·2	— 3·0	— 5·0	3
4	— 3·0	— 4·2	— 2·4	— 3·2	4
5	+ 5·4	— 6·6	+ 5·4	+ 1·4	5
6	+ 2·0	+ 1·2	+ 3·0	+ 5·4	6
7	+ 9·0	+ 4·2	+ 9·6	+ 7·6	7
8	+ 19·6	+ 8·4	+ 7·8	+ 8·6	8
9	+ 7·2	+ 6·6	— 0·9	+ 4·3	9
10	+ 3·0	+ 7·2	— 1·8	+ 2·8	10
11	— 3·6	— 1·2	— 4·2	— 3·0	11
12	— 4·8	— 9·0	— 18·0	— 10·6	12
13	— 3·0	— 13·2	— 15·0	— 10·4	13
14	— 3·0	— 8·4	— 9·6	— 7·0	14
15	— 7·2	— 3·6	+ 4·2	— 2·2	15
16	+ 3·0	+ 3·6	+ 7·8	+ 4·8	16
17	+ 7·8	+ 9·6	+ 13·8	+ 10·4	17
18	+ 7·8	+ 14·4	+ 17·4	+ 13·2	18
19	+ 4·8	+ 18·0	+ 15·0	+ 12·6	19
20	+ 3·0	+ 12·6	+ 6·0	+ 7·2	20
21	— 2·4	+ 18·6	+ 2·4	+ 6·2	21
22	— 7·8	+ 9·6	— 3·0	— 0·4	22
23	— 6·0	+ 5·4	— 3·6	— 1·4	23

In conclusion, it may be useful to call the attention of the Society, and of those Fellows in particular who interest themselves in tracing up the phenomena of nature to their physical causes, to the assemblage of facts which are now available for such inquiries, in a branch of magnetical science which may not inappropriately be called *celestial magnetism*. In the introductory discussion prefixed to the 2nd volume of the St. Helena Magnetical Observations, p. cxliv to cxlviii, the lunar-diurnal variation is given for each of the three magnetic elements, the Declination, the Dip, and the Intensity of the force, at the four stations of Toronto, St. Helena, the Cape of Good Hope and Hobarton, and for the Declination at two additional stations Kew and Pekin. The variations are given both in formulæ and in tables; the latter exhibiting the amount of the lunar influence at each of the 24 lunar hours, in the several magnetic elements at each station. These data are directly applicable to inquiries into the nature of the moon's magnetism; and into the mode by which the moon's magnetism acts either on the magnetism of the earth itself, or on the magnetic needle stationed at different points of the earth's surface, so as to produce a small but systematic and perfectly appreciable variation in each of the magnetic elements, having a double period in every lunar day.

The lunar-diurnal variation of the Declination at Kew and Hobarton, as given in this communication, is slightly different from the figures in the 2nd St. Helena volume referred to, because the results at Kew are a mean of 3 years instead of 2, as in the St. Helena volume; and at Hobarton a lower standard has been taken for the disturbances, causing a larger number of the disturbed observations to be omitted in the calculation of the lunar-diurnal variation.

*January 17, 1861.*

Major-General SABINE, R.A., Treasurer and Vice-President,  
in the Chair.

The following communication was read :—

“ On the Homologies of the Eye and of its Parts in the Invertebrata.” By J. BRAXTON HICKS, M.D. Lond., F.L.S. Communicated by JOHN LUBBOCK, Esq. Received December 20, 1860.

(Abstract.)

The author first remarks, that the great similarity which exists in the parts of the eye throughout all classes of the Vertebrata, coupled with the desire to find in the eyes of all animals the same component parts as in that great group, has militated much against a proper understanding of the different parts of the invertebrate eye.

A different method has been followed in the observations upon which the present communication is founded ; for, starting from the simplest condition in which the organ appears in the animal series, the necessary elements of a picture-seeing eye are first determined, and afterwards those parts which are superadded to it. A distinction is then drawn between the mere light-seeing eye and that which perceives a picture ; the former requiring no lens, the latter having one. The elements of the picture-seeing eye are, in fact,—1st, a nerve-fibre, and bulb ; 2ndly, a cell possessing more or less refractive power, and resting on the nerve-bulb ; 3rdly, pigment ; 4thly, the nerve-sheath, including the other structures. The homologies of these parts are then considered, and traced through the various classes. The cell resting on the nerve-bulb is the homologue of the crystalline lens. It has the power of secreting into its interior a highly refractive substance, which, in the Invertebrata generally, is semifluid, but sometimes, as in certain Insecta, solid. The bilateral tendency of the cell in *Asterias* and in some Entomostraca is pointed out, as also the tendency of the solid lens of insects to split longitudinally into four portions. This is compared with the same condition in the more fluid lens of the decapod crustaceans, and referred

to the mode of development as set forth by Dr. E. Claparède. Moreover, as in many cases these cells become so fused as to form an entirely homogeneous body, the author thinks that the single crystalline lens, possessing many nerve-bulbs (as in mollusks), is formed really by the combination of their various lenses; the pigment in this case being confined to the nerve-bulb, which is its normal position.

It is then pointed out, for the first time, that in some insects, and in *Iulus*, the lens possesses a very slight refractive power, being filled with a fluid as little refractive as water. This is more particularly the case in the *Diptera*, where the dermal facets become so spherical as to usurp the function of the lens, while an example is furnished by the eye of *Aromia moschata* of the complete disappearance of the lens. In opposition to the opinion entertained by many naturalists, the author maintains that each bulb, provided with a refracting body, is capable of perceiving a picture independently of the assistance of other adjoining lenses.

The homology of the nerve-sheath is traced from its condition as a delicate membrane which passes over the simple eye, to that of the sclerotic and cornea; and it is shown to constitute the *true* cornea. After referring to the pigment and iris, the author discusses the parts added to the simple eye. The *anterior chamber*, or the space found anteriorly to the lens and iris (where the latter exists), first appears in the leeches, but is a very variable element in the Invertebrata, existing in some aquatic animals, as leeches and cephalous mollusks, while it is absent in others, as *Paludina*, *Limnæa*, &c.; and thus the opinion held by some, that an anterior chamber is of little use in aquatic animals, as in fishes, is scarcely tenable, because they have a compensation in the globular lens. The author has never found an anterior chamber in the compound eyes of the Insecta, although a space has been described, and erroneously called a lens. Still the possibility of such a space is not thereby denied; only, should it exist, it would be homologous to the anterior chamber, and not to the lens; and the transparent body behind it is the true lens, and not a vitreous humour, as is often represented. There is such a chamber in the Decapods, in which it is well marked. In regard to a chamber posterior to the lens, the author is of opinion that none is to be found below the mollusks, and points out that what

has been so called in the ocelli and stemmata is not the homologue of the vitreous humour, but probably that of the lens, which otherwise is absent in these eyes, for the lens of the ocelli is shown below to be of dermal origin.

The next portion of the paper is occupied in pointing out the part which the integument plays in regard to the optical organs where it passes over them ; and it is shown that in all of the Invertebrata the integument becomes more or less subservient to the functions of those organs. It is in animals with a dermo-skeleton that the greatest subjection of the integument to the eye is seen ; and the change is reviewed *gradatim* from where the eye is independent of the integument, as in *Daphnia*, through *Gammarus*, where the integument is merely applied to it, till, in *Artemia Salina*, we find the inner surface indented, but not yet faceted. In *Branchipus* the inner layer only becomes faceted, till in the Decapods both layers become so. But it is in the Insecta that the change is most marked, the form of the facets becoming more or less lens-like, till in Diptera they are nearly perfect spheres. Their power of refraction is very high ; they are supplied with corrections for spherical and chromatic aberration, and they almost altogether supply the place of the true crystalline lens, as before pointed out. The ocelli in Insects and Arachnida show this condition carried to the extreme ; and the author conceives that the so-called crystalline lens of these organs is derived entirely from the integument : this he endeavours to show by reference to their structure and intimate connexion with the other dermal layers, and by tracing them through the various tribes ; and a more complicated structure is described, for the first time, in the median eye of one of the Scorpionidæ (*Buthus*), where the inner integumental layer is converted into a lens more distinct than in any other of the Arachnida. A chamber is also shown to exist in it, formed by the separation of the epidermis from the other layers, producing a plano-convex lens similar in form to the anterior chamber of the higher animals, and analogous to it in function. The stemmata and ocelli, like the facets of the compound eye, have peculiarities in their structure for the correction of spherical and chromatic aberration.

The author concludes by pointing out the striking homology existing throughout the whole animal world in those parts which

were pointed out at the commencement of the paper as forming the essentials of an eye, and considers that it affords a contradiction to the position of Agassiz, "That every great type is formed on a distinct plan,—so peculiar, indeed, that homologies cannot be extended from one type to the other, but are strictly limited to each of them." On the contrary, the eye of an *Asterias* is formed on the same plan as that of a *Planaria* and a *Daphnia*; and the eye of the leech possesses the same parts as that of the *Helix*, while the similarity of the eye of the cephalopod to that of the vertebrata is obvious to all.

January 24, 1861.

Major-General SABINE, R.A., Treasurer and Vice-President,  
in the Chair.

The following communications were read :—

- I. "On the Calculus of Symbols, with Applications to the Theory of Differential Equations." By W. H. A. RUSSELL, A.B. Communicated by ARTHUR CAYLEY, Esq. Received December 20, 1860.

(Abstract.)

The calculus of generating functions, discovered by Laplace, was, as is well known, highly instrumental in calling the attention of mathematicians to the analogy which exists between differentials and powers. This analogy was perceived at length to involve an essential identity, and several analysts devoted themselves to the improvement of the new methods of calculation which were thus called into existence. For a long time the modes of combination assumed to exist between different classes of symbols were those of ordinary algebra; and this sufficed for investigations respecting functions of differential coefficients and constants, and consequently for the integration of linear differential equations, with constant coefficients. The laws of combination of ordinary algebraical symbols may be divided into the commutative and distributive laws; and the number of symbols in the higher branches of mathematics, which are commutative with respect to one another, is very small. It became

then necessary to invent an algebra of non-commutative symbols. This important step was effected by Professor Boole, for certain classes of symbols, in his well-known and beautiful memoir published in the Transactions of this Society for the year 1844; and the object of the paper which I have now the honour to lay before the Society is to perfect and develop the methods there employed.

For this purpose I have constructed systems of multiplication and division for functions of non-commutative symbols, subject to the same laws of combination as those assumed in Professor Boole's memoir; and I thus arrive at equations of great utility in the integration of linear differential equations with variable coefficients.

I then proceed to develop certain general theorems, which will, I hope, be found interesting. I have applied the methods of multiplication, as just explained, to deduce theorems for non-commutative symbols analogous to the binomial and multinomial theorems of ordinary algebra.

Lastly, I have shown how to employ the equations deduced in the earlier part of this paper in the integration of linear differential equations. I have, for this purpose, made use of methods closely resembling the method of divisors which has so long been used in resolving ordinary algebraical equations. The whole paper will, I hope, be found to be a step upwards in the important subject of which it treats.

I shall just observe, that the symbolical combinations used in this paper may also be applied to the calculus of finite differences, as may be seen in Professor Boole's memoir.

## II. "On the Properties of Liquid Carbonic Acid." By GEORGE GORE, Esq. Communicated by Professor TYNDALL. Received January 17, 1861.

(Abstract.)

In this communication the author has shown how a small quantity of liquid carbonic acid may be readily and safely prepared in glass tubes closed by stoppers of gutta percha, and be brought in a pure state into contact with any solid substance upon which it may be desired to ascertain its chemical or solvent action, or be submitted



to the action of electricity by means of wires introduced through the stoppers.

By immersing about fifty substances in the liquid acid for various periods of time, he has found that it is comparatively a chemically inert substance, and not deoxidized by any ordinary deoxidizing agent except the alkali-metals. Its solvent power is extremely limited; it dissolves camphor freely, iodine sparingly, and a few other bodies in small quantities; it does not dissolve oxygen-salts, and it does not redden solid extract of litmus; it penetrates gutta percha, dissolves out the dark-brown colouring matter, and leaves the gutta percha undissolved, and much more white. It also acts in a singular and somewhat similar manner upon india-rubber; the india-rubber whilst in the liquid acid exhibits no change, but immediately on being taken out it swells to at least six or eight times its original dimensions, and then slowly contracts to its original volume, evidently from expansion and liberation of absorbed carbonic acid; and it is found to be perfectly white throughout its substance. These effects upon gutta percha and india-rubber may prove useful for practical purposes.

The liquid acid is a strong insulator of electricity; sparks (from a Ruhmkorff's coil) which would pass readily through  $\frac{2}{3}$ nds of an inch of cold air, would with difficulty pass through about  $\frac{1}{10}$ th of an inch of the liquid acid.

In its general properties it is somewhat analogous to bisulphide of carbon, but it possesses much less solvent power over fatty substances.

*January 31, 1861.*

Major-General SABINE, R.A., Treasurer and Vice-President,  
in the Chair.

Sir William Jardine, Bart., was admitted into the Society.

The following communications were read:—

- I. "On Systems of Linear Indeterminate Equations and Congruences." By H. J. STEPHEN SMITH, Esq., M.A., Fellow and Mathematical Lecturer of Balliol College, Oxford. Communicated by Professor J. J. SYLVESTER. Received January 17, 1861.

(Abstract.)

The present communication relates to the theory of the solution, in positive and negative integral numbers, of systems of linear indeterminate equations, having integral coefficients. In connexion with this theory, a solution is also given of certain problems relating to rectangular matrices, composed of integral numbers, which are of frequent use in the higher arithmetic. Of this kind are the two following:—

1. "Given (in integral numbers) the values of the determinants of any rectangular matrix of given dimensions, to find all the matrices, the constituents of which are integers, and the determinants of which have those given values.

2. "Given any rectangular matrix, the determinants of which have a given number  $D$  for their greatest common divisor, to find all the supplementary matrices, which, with the given matrix, form square matrices, of which the determinant is  $D$ ."

A solution of particular, but still very important cases of these two problems, has been already given by M. Hermite. The method by which in this paper their general solution has been obtained, depends on an elementary, but apparently fertile principle in the theory of indeterminate linear systems; viz. that if  $m$  be the *index of indeterminateness* of such a system (*i. e.* the excess of the number of indeterminates above the number of really independent equations), it is always possible to assign a set of  $m$  solutions, such that the determinants of the matrix formed by them shall admit of no common divisor but unity.

Such a set of solutions is termed a *fundamental set*, and possesses the characteristic property, that every other solution of the system can be integrally expressed by means of the solutions contained in it. A set of *independent* solutions is one in which the determinants of the matrix have a finite common divisor, *i. e.* are not all zero.

The theory of independent and fundamental sets of solutions in some respects resembles that of independent and fundamental systems of units in Lejeune Dirichlet's celebrated generalization of the solution of the Pellian equation.

By the aid of the same principle of fundamental sets, the following criterion is obtained for the resolubility or irresolubility of indeterminate linear systems.

"A linear system is or is not resolvable in integral numbers, according as the greatest common divisor of the determinants of the matrix of the system is or is not equal to the corresponding greatest common divisor of its *augmented* matrix."

[The matrix of a linear system of equations is, of course, the rectangular matrix formed by the coefficients of the indeterminates; the *augmented* matrix is the matrix derived from that matrix, by adding to it a vertical column composed of the absolute terms of the equations.]

A system of linear congruences may, of course, be regarded as a system of linear indeterminate equations of a particular form; and the criterion for its resolubility or irresolubility is implicitly contained in that just given for any indeterminate system. But this criterion may be expressed in a form in which its relation to the modulus is very clearly seen.

Let

$A_{i1}x_1 + A_{i2}x_2 + \dots + A_{in}x_n \equiv A_{i,n+1} \pmod{M}$ ,  $i=1, 2, 3, \dots, n$   
represent a system of congruences; let us denote by  $\nabla_n, \nabla_{n-1}, \dots, \nabla_1, \nabla_0$ , the greatest common divisors of the determinant, first minors, &c., of the matrix of the system [so that, in fact,  $\nabla_n$  is the determinant itself,  $\nabla_1$  the greatest common divisor of the coefficients  $A_{ij}$ , and  $\nabla_0=1$ ]; by  $D_n, D_{n-1}, \dots, D_1, D_0$  the corresponding numbers for the *augmented* matrix; let also  $\delta_i$  and  $d_i$  respectively represent the greatest common divisors of  $M$  with  $\frac{\nabla_i}{\nabla_{i-1}}$ , and of  $M$  with  $\frac{D_i}{D_{i-1}}$ ; and put

$$m = \delta_n \times \delta_{n-1} \times \dots \times \delta_1,$$

$$\mu = \delta_n \times d_{n-1} \times \dots \times d_1, \dots$$

Then the necessary and sufficient condition for the resolubility of the system is

$$m = \mu;$$

and when this condition is satisfied, the number of solutions is precisely  $m$ .

The demonstration of this result (which seems to exhaust the theory of these systems) is obtained by means of the following theorem:—

“ If  $\|A\|$  represent any square matrix in integral numbers,  $\nabla_n$  its determinant,  $\nabla_{n-1}, \nabla_{n-2}, \dots, \nabla_1, \nabla_0$  the greatest common divisors of its successive orders of minors, it is always possible to assign two unit-matrices  $\|a\|$  and  $\|\beta\|$ , of the same dimensions as  $\|A\|$ , and satisfying the equation

$$\|A\| = \|a\| \times \left\| \begin{array}{cccccc} \frac{\nabla_n}{\nabla_{n-1}}, & 0, & 0, & \dots & 0 \\ 0, & \frac{\nabla_{n-1}}{\nabla_{n-2}}, & 0, & \dots & 0 \\ 0, & 0, & \frac{\nabla_{n-2}}{\nabla_{n-3}}, & \dots & 0 \\ . & . & . & . & . \\ 0, & 0, & . & . & \frac{\nabla_1}{\nabla_0} \end{array} \right\| \times \|\beta\|.”$$

The following result (among many which may be deduced from this transformation of a square matrix) admits of frequent applications:—

“ If  $D$  be the greatest common divisor of the determinants of the matrix of any system of  $n$  independent linear equations; of the  $D^n$  sets of values (incongruous mod.  $D$ ) that may be attributed to the absolute terms of the equations, the system is resolvable for  $D^{n-1}$ , and irresolvable for  $D^{n-1} (D-1)$ .”

As an example of the use that may be made of this result, it is shown, in conclusion, that it supplies an immediate demonstration of a fundamental principle in the general theory of complex integral numbers, composed of the root of any irreducible equation, having its first coefficient unity, and all its coefficients integral; viz. that the number of incongruous residues, for any modulus, is always represented by the norm of the modulus. A demonstration of this principle has, however, already been given in the ‘Quarterly Journal of Pure and Applied Mathematics,’ in a paper signed *Lanavicensis*; to whom, therefore, the honour of priority in this inquiry is due.

II. "Contributions to the Physiology of the Liver—Influence of Alkalies." By FREDERICK W. PAVY, M.D. Communicated by Dr. SHARPEY, Sec. R.S. Received January 24, 1861.

I have already communicated to the Royal Society the fact that the introduction of an alkali, as the carbonate of soda, into the circulation, prevents the production of diabetes in an animal after an operation on the sympathetic nerve that otherwise occasions it. On excising, for example, the superior cervical ganglia, or dividing the ascending branches of the superior thoracic ganglia, diabetes is an almost constant result in the dog. Now, I have a record of ten experiments, in which, before attacking the superior cervical ganglia, 200 grains, or nearly so, of the crystallized carbonate of soda dissolved in a small quantity of water, were introduced into the general circulation through the jugular vein. In not one of these cases was there any diabetes produced. In another instance, where I employed only 100 grains of the carbonate of soda, the urine soon after the operation on the ganglia became decidedly, although not strongly saccharine. On dividing the ascending branches of the superior thoracic ganglia also, after the introduction of 200 grains of carbonate of soda, I have failed to meet with sugar in the urine. Under this injection of 200 grains of carbonate of soda into the circulation, the urine becomes exceedingly copious in quantity, pale-coloured, and alkaline to test-paper, and effervesces strongly on the addition of an acid.

Although the introduction of carbonate of soda into the system thus counteracts the influence of certain lesions of the sympathetic leading to the production of sugar (I presume) in the liver, yet it is not sufficient to prevent the appearance of sugar in the urine where life has been destroyed and the circulation is kept up artificially. On resorting to artificial respiration immediately after death, the action of the heart being kept up, the sugar which is formed in the liver, as the result of a *post-mortem* occurrence, escapes from it into the blood, and in passing through the kidneys is eliminated by these organs. In two experiments I introduced 200 grains of carbonate of soda into the jugular vein previous to the destruction of life by

pithing. The circulation was maintained by artificial respiration; an enormous flow of alkaline urine took place, which in both cases presented a saccharine character.

From another experiment, however, it is evident that the carbonate of soda, if it arrives at the liver in sufficient quantity, holds in check the tendency of hepatine to pass into sugar, and thus prevents the production of saccharine urine when the circulation is kept up artificially after death. Introduced into the general venous system, as in the two experiments mentioned, the amount of the carbonated alkali that reached the liver was insufficient to counteract the ordinary *post-mortem* change. Introduced into a branch of the portal system, it must all pass to the liver, and accordingly the result is altered. I injected 200 grains of carbonate of soda, dissolved in an ounce of water, into a branch of the portal vein of a dog. The animal was killed by destroying the medulla oblongata. Artificial respiration was performed for an hour and three quarters, during the whole of which time the heart continued beating vigorously. The urine contained in the bladder at the end of the experiment was pale-coloured and alkaline, and effervesced slightly on the addition of an acid. It did not give evidence of even a trace of sugar. The blood which escaped on opening the body behaved precisely as it does under natural circumstances, and gave just a trace of reaction with the copper solution. The liver, examined a short time after removal, was quite devoid of sugar. The presence of hepatine, however, was easily recognized, and in the course of twenty-four hours sugar had been produced, so as to give a copious orange-red reduction with the copper solution.

Looking to these facts, I determined to investigate the effects of an alkali on the hepatine in the liver during life. I had long since noticed that the carbonate of potash introduced into the stomach caused a disappearance of hepatine in a short space of time without the production of sugar. I had injected 200 grains dissolved in two ounces of water, through the œsophagus into the stomach of a dog that had recently been fed. An examination of the blood before and at intervals after the injection, gave no sign of any difference being produced. The animal died during the night; and from its condition in the morning it was probable that death had taken place within twelve hours after the injection. The liver was much altered

from its natural appearance, and did not contain a trace of either hepaticine or sugar.

In other experiments, where carbonate of soda was introduced into the stomach and duodenum, I found that when the animal had lived several hours, the hepaticine had entirely disappeared; but when it was killed within three or four hours, some amount of hepaticine remained.

Absorption having had to take place, the passage of the carbonated alkali to the liver can only have been comparatively gradual. On the other hand, the rapid distribution of it in tolerable quantity through the organ, as by injecting it into a branch of the portal vein, causes a transformation of hepaticine into something else than sugar in an astonishingly short period of time. I regard the discovery of this fact as a most important step in our study of the physiological history of hepaticine, and believe that it is not unlikely to lead us to a knowledge of its natural destination in the economy.

In proof of the effect produced by the injection of a solution of carbonate of soda into the portal system during life, I will give the leading features of the first three experiments that I performed.

In the first, sixty grains of carbonate of soda dissolved in five drachms of water were injected into one of the mesenteric veins of a healthy, nearly full-grown pup. Between one and a half and two hours afterwards the animal was killed by pithing. The liver was much swollen, dark-coloured from congestion, and easily lacerable. The walls of the gall-bladder were thickened from cedema, and the tissue in the neighbourhood of the transverse fissure of the liver was also quite cedematous. The liver contained neither hepaticine nor sugar. The blood collected from the right side of the heart was also quite free from sugar.

In the second experiment, fifty grains of the carbonate, dissolved in four drachms of water, were employed as the injection. In performing the operation, the stomach was observed to contain food, and the lacteals to be well injected with chyle. Four hours afterwards the dog was pithed. The liver was devoid of both hepaticine and sugar. The blood from the right side of the heart was likewise devoid of sugar. The urine found in the bladder presented an alkaline reaction, and effervesced slightly on the addition of an acid; like the liver and blood, it was quite devoid of sugar. In the third,

80 grains, dissolved in  $6\frac{1}{2}$  drachms of water, constituted the injection used. In one hour's time the dog was killed. The liver was dark-coloured and exceedingly congested, fluid blood oozing out of it in large quantity when it was sliced. Neither hepatine nor sugar was present.

Here, then, were the experiments which taught me that, under the influence of the presence of a certain quantity of carbonate of soda, the whole of the hepatine in the liver might be destroyed in a short space of time during life, without any appearance of the production of sugar. The animals submitted to these experiments were perfectly good-conditioned and healthy, and under such circumstances hepatine is always found to some considerable extent in the liver. There could not be the slightest doubt that hepatine would have been met with had not the carbonate of soda exercised a special action in promoting its rapid disappearance or transformation. In one of the experiments, it will be observed that this had been entirely effected in an hour.

From many subsequent experiments I learnt that, to obtain the result I have mentioned, certain circumstances require to be attended to in making the injection. In some experiments I did not meet with a similar total disappearance of hepatine, although even a larger quantity of carbonate of soda had been employed. I subsequently found that this depended on the circumstance that, when the injection is made slowly, and the blood is flowing freely through the system, the carbonate of soda passes in great part through the liver without effecting a total disappearance of its hepatine or materially altering its natural aspect; whereas, when the carbonated alkali is rapidly thrown in, it seems to cause an obstruction to the flow of blood through the organ, and to exude into, and thus be detained in its tissue. This may be seen on watching the liver whilst the injection is being effected; a number of small light-coloured spots, constituting the lobules influenced by the injection, first make their appearance, and the liver rapidly becomes dark-coloured, swollen, and turgid with blood. It may still, however, allow the blood to move through it; but in some cases, in which I have used a large quantity of the carbonate in a concentrated state, 200 grains for instance in an ounce of water, the circulation through the liver has evidently become stopped, for the chylo-poietic viscera have presented exactly the appearance



as if the portal vein had been obstructed by ligature or otherwise. Holding the muzzle of the animal so as temporarily to check its breathing, and so impede the escape of blood from the liver during the introduction of the solution, has the same effect as injecting it rapidly. The carbonate of soda is delayed in the liver so as to bring about the chemical and physical alteration I have noticed.

If, then, the conditions of the experiment are such as to conduce to the free passage of the injected solution through the liver, the hepatine escapes the influence requisite to cause its rapid and complete disappearance. But, should the circumstances be favourable to the saturation of the parenchyma of the liver with the carbonated alkali, the hepatine is so rapidly transformed that it soon ceases to be discoverable.

In the experiments mentioned, the hepatine completely disappeared in the space of an hour ; a much shorter time, however, than this is required ; for I have found that even within a few minutes of the injection being completed the liver has been totally deprived of hepatine. Thus, in one experiment, 200 grains of carbonate of soda in one ounce of water were injected into a branch of the portal vein of a good-sized, good-conditioned dog that had been fed about twenty hours previously ; as soon as the vein was ligatured, the animal being kept under the influence of chloroform, one of the lobes of the liver was tied at its base and removed ; examined within a very short time afterwards, it was found perfectly free from both hepatine and sugar. In another experiment, 100 grains of carbonate of soda in half an ounce of water were injected into one of the mesenteric veins of a large, exceedingly good-conditioned dog that had been likewise fed about twenty hours previously. When the injection was completed, a portion of the liver was obtained and examined ; it was quite devoid of hepatine, but contained just a discoverable trace of sugar ; this probably existed in the liver before the injection, and was formed in consequence of the congestion to which the organ was purposely submitted by temporarily obstructing the breathing.

I have tried the substitution of the caustic for the carbonated alkali, but there are accidental circumstances occasioned by its use which render it unfit for employment. Fifty grains of caustic potash were dissolved in one ounce of water and introduced into a branch of the portal vein. After the injection was completed and the vein

secured, hæmorrhage of dark pitchy blood took place from a vessel which gave way through the action of the alkali upon it. This was very soon checked, and the animal lived, although in a comatose state, for about an hour. Upon examining the liver after death, it was found that it had not been uniformly penetrated by the alkali, and that the large veins were plugged up with coagulated or solidified blood, which had arrested the portal circulation; I have never observed a similar solidification of the blood produced by the use of the carbonate of soda. My analysis of the liver a short time after death indicated the presence of both hepatine and sugar. It was quite evident from the appearance of the organ, that much of its tissue had not been reached by the injection, and therefore escaped the action of the alkali.

It having been thus shown that a rapid disappearance of hepatine may be occasioned by the presence of the carbonate of soda in the liver during life, it remains now to determine the nature of the process of disappearance, and to discover the material into which the hepatine is transformed. I am not in a position as yet to enter satisfactorily into a discussion of this question; but, if I cannot state how and into what the hepatine is thus changed—if I cannot at present speak definitely upon the positive side of the question, I, however, can advert to its negative aspect, and say what the process is not; and, in following this course, I am adopting a rational proceeding, for it is from a consideration of the known that we are placed in the best position for making a profitable advance towards the unknown.

As hepatine is a body presenting such characteristic reactions, and so easily susceptible of recognition, we can have no difficulty in deciding that it is really absent or transformed in the circumstances stated, and that it is not merely masked or concealed by the material employed for injection. It is the property of hepatine to be thrown down as a white precipitate by spirit. The alkalies and carbonated alkalies, for certain, do not prevent this precipitation from taking place. A solution of hepatine presents a most striking lactescence; and by this character, used with precaution, not only may the question of its presence or absence be decided, but, when present, its relative amount may be pretty accurately judged of in different specimens. With iodine it gives a strong deep-red coloration. Lastly,

boiled with a little sulphuric acid, or treated when in a neutral state with saliva, at a moderately elevated temperature, it undergoes transformation into sugar. Now it is not upon the evidence of one, but of all these tests, that I have relied.

Does the hepatine disappear by being metamorphosed into sugar? If so, taking an ordinary sized liver, say  $7\frac{1}{2}$  ounces in weight, containing an average amount of hepatine for an animal diet, say seven per cent. ; and reckoning (in accordance with my analyses showing the relation of loss of hepatine to gain of sugar in the liver after death) that the loss of one and a half part of the former is accompanied with the production of one part of the latter, then upwards of 150 grains of sugar have to be accounted for. Under such circumstances sugar ought to be easily detected in quantity in the liver and blood ; or else it must be assumed to be decomposed as fast as it is formed. But, as I have mentioned, the liver in my experiments has been found free from sugar as well as hepatine, and the blood has exhibited a similar state, or has only been charged to a slight extent—a condition that has probably existed previous to the injection, and been produced by the preliminary part of the experiment, chloroform having been always used to occasion anæsthesia. The sugar, then, is not to be discovered, and we shall find the other hypothesis to be equally untenable.

When sugar is present either in the liver or blood, I have failed to discover that the introduction of a carbonated alkali exercises any perceptible influence over it. It is true that, in order to avoid error, certain precautions are necessary in preparing a liquid from an alkaline specimen for the application of the copper test. If the decoction of the liver is allowed to remain only in a slight degree alkaline, a certain amount of albuminous matter is retained in solution, which interferes materially with the action of the test. If acetic acid be used to neutralize, a small excess of this will produce the condition that it was intended to avoid. I find, however, that citric acid is not liable to this objection ; and, accordingly, if it be added in slight excess to a specimen of alkaline liver, a decoction can be prepared in which the slightest amount of sugar or of hepatine may be easily and certainly recognized.

To show that when sugar is formed the carbonate of soda has no power of destroying it, I may refer to the following experiment, which

accords with my general experience. Into the liver of a small dog, about ten minutes after the destruction of its life, 100 grains of carbonate of soda in half an ounce of water were injected through the portal vein; in a couple of hours an examination was made; the decoction of liver, prepared as I have recommended, gave the usual orange-red reduction with the copper-solution that is observed where the *post-mortem* change has been allowed to take place. Should hepatine in fact be transformed into sugar, the latter will unquestionably be discoverable, notwithstanding the presence of an alkali or alkaline carbonate.

I have tried the effect of injecting grape-sugar along with carbonate of soda into the liver during life, and the result was in strict accordance with what I have just stated. 200 grains of each were dissolved together in an ounce of water, and injected into one of the mesenteric veins of a good-sized healthy dog; some blood taken from the carotid artery after the operation was found to be charged with sugar to a large extent. In one hour's time the animal was killed. The liver and blood were both found saccharine. The urine contained in the bladder was mixed with blood, which I have observed always to be the case when such an amount as 200 grains of the carbonate of soda has been employed. After being boiled and filtered, it was tested with the copper solution, and gave a strong reaction of the presence of sugar.

The evidence before us tends, I think, satisfactorily to prove that the hepatine is neither concealed nor transformed into sugar when lost sight of after injection of the carbonate of soda. Again, it does not seem that its disappearance can be owing to any direct chemical action of the carbonated alkali. It is one of the most striking properties of hepatine to resist the action of even the caustic alkali at a boiling temperature, and I certainly have not been able to perceive that the carbonated alkali is capable of exercising any direct chemical influence upon it up to a boiling heat.

Looking to all the facts in my possession, I am inclined to believe, although I cannot at present substantiate the opinion, that the rapid disappearance of hepatine which has been noticed, is due to one of those catalytic actions of which we have such numerous examples occurring amongst the phenomena of life. It is well known that sugar, under the influence of certain catalytic conditions, is transformed

into alcohol and carbonic acid, and under others into lactic acid. Now, may not hepatine occupy a parallel position, and be susceptible of undergoing a process of metamorphosis besides that into sugar, the product having as yet escaped discovery? Hepatine itself is a body that has been known to us but a very short period.

I have mentioned that the carbonate of soda does not enjoy any direct chemical power of effecting a destruction of hepatine up to the temperature of ebullition; neither, as far as I have yet learnt, can it occasion a disappearance of it from the liver after death: it, however, holds the saccharine metamorphosis in check. Previous to this inquiry, although I had known that a tolerably strong solution of the caustic alkali prevented the production of sugar in the liver after death, I was not aware that a moderate amount of the carbonated alkali has so completely the same effect. In an experiment recently performed, I injected, instantly after the animal was pithed, 200 grains of the carbonate of soda dissolved in an ounce of water, into the liver through the portal vein: a couple of lobes happened to escape being properly penetrated by the injection, and in this part of the liver the ordinary *post-mortem* production of sugar took place. In the other portion of the organ no saccharine metamorphosis occurred; nor could I discover that the injection had in any degree caused a disappearance of the hepatine, as happens under its operation during life. The liver examined shortly after the introduction of the carbonate of soda had been completed, was found to be highly charged with hepatine and quite devoid of sugar; examined again on the following day, the result was identically the same.

Amongst my examinations of the livers of the lower animals, I once met with a specimen from a cod-fish in which the hepatine resisted the ordinary *post-mortem* transformation into sugar. Hepatine was present in the greatest abundance, but there was only the merest trace of sugar discoverable when first examined, and likewise after it had been standing aside for twenty-four hours. It was then exposed to a moderately elevated temperature for three hours, and still indicated only a trace of the presence of sugar, notwithstanding that the decoction immediately produced sugar copiously on being treated with saliva. The result being so much at variance with that usually met with, made a strong impression upon me. I can only see two ways of accounting for it; either the hepatine was protected

from the influence of catalytic action in some such manner as in my experiment with the injection of carbonate of soda into the liver instantly after death, or the absence of change depended upon a want of the requisite catalytic agent.

I cannot help regarding it as a most significant fact, which, however, I merely mention here, without at present commenting upon it, that after the introduction of carbonate of soda into the system, the liver gives upon analysis so high a per-centage of fat. The usual quantity of fat in the liver of the healthy dog kept upon an animal diet, I have found to be about six per cent. It is seldom much more, and often only three or four per cent. After the introduction of carbonate of soda into the system, my analyses have given me from ten to twelve and thirteen, and even more than this per cent. The employment of large quantities of the carbonate does not seem to have the effect I have just mentioned. It is only where the liver is injected with a moderate amount that the high per-centage of fat is noticeable: I do not think that the hepatine is directly transformed into fat, but there is, I have strong reason to believe, a close connexion between the two; at all events, it is a point that I have grounds for endeavouring to work out. The formation of the bile also, from what I have observed, appears to be involved in this question, and forms a subject of consideration in the investigation I am conducting.

The conclusions advanced in this communication are:—

“ That the introduction of carbonate of soda into the circulation prevents the production of saccharine urine after lesions of the sympathetic nerve otherwise occasioning it.

“ That carbonate of soda injected into the general venous system does not prevent the urine from becoming saccharine after the destruction of life when the circulation is kept up artificially; but injected into the portal system, so that all may pass into the liver, it has the effect of keeping the urine entirely free from sugar.

“ That carbonate of soda injected into the portal system during life, causes a rapid disappearance of hepatine from the liver without any sign of the production of sugar.

“ That in the disappearance of hepatine under the influence of the carbonate of soda, the hepatine is not concealed, nor transformed into sugar, nor destroyed by any direct chemical power possessed by the carbonated alkali. The facts before me would suggest that it is

transformed by a process of the nature of catalysis, the product having as yet escaped discovery.

“That carbonate of soda injected into the liver after death does not effect a disappearance of hepatine, but even in moderate quantity holds the saccharine metamorphosis completely in check.

“That there is probably a close connexion between the disappearance of hepatine, the production of fat, and the state of the bile.”

*February 7, 1861.*

Major-General SABINE, R.A., Treasurer and Vice-President,  
in the Chair.

The Right Hon. the Earl of Ellesmere and Professor Harkness were admitted into the Society.

The BAKERIAN LECTURE was then delivered by Professor TYNDALL, F.R.S., “On the Absorption and Radiation of Heat by Gases and Vapours, and on the Physical Connexion of Radiation, Absorption, and Conduction.”

The Lecturer gave an account of the researches which form the subject of a paper with the above title, communicated by him to the Society; and in explaining the methods followed, he exhibited the apparatus which he had employed in his experiments. The following is an abstract:—

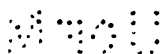
(Abstract.)

The apparatus made use of in this investigation consists of the following parts:—

1. A copper cube C, containing water kept constantly boiling, and one of whose faces, coated with lampblack, forms the source of radiant heat.

2. A brass tube 2·4 inches in diameter, which is divided into two portions,  $\alpha$  and  $\beta$ .

$\alpha$ . The portion of the tube intended to receive the gases and vapours; it is stopped air-tight at its two ends by plates of transparent rock-salt, and is attached to a good air-pump, by which it can be exhausted at pleasure. The length is 4 feet.



$\beta$ . An air-tight chamber between the tube  $\alpha$  and the cube C. It is kept constantly exhausted; and the calorific rays, therefore, pass from the radiating plate through a vacuum into the tube  $\alpha$ , thus retaining the quality which belonged to them at the moment of emission.

To prevent the transmission of heat by conduction from the cube C to the tube  $\alpha$ , the chamber  $\beta$  is partly embraced by an annular space in which cold water continually circulates.

3. A thermo-electric pile furnished with two conical reflectors, and connected with an excellent galvanometer\*. One of the faces of the pile receives the rays which have passed through the tube  $\alpha$ .

4. A second copper cube C', also filled with boiling water, and whose rays fall upon the second face of the thermo-electric pile. The two cubes C and C' thus radiating upon the opposite faces of the pile, tend, of course, to neutralize each other.

Between the cube C' and the adjacent face of the pile, a screen S is introduced, being attached to an apparatus of Ruhmkorff, capable of extremely fine motion; by the partial advance or withdrawal of this screen the two sources of heat can be caused to neutralize each other perfectly.

The tube  $\alpha$  and the chamber  $\beta$  being both exhausted, the needle is brought exactly to zero by means of the screen S. The gas or vapour to be experimented with is now admitted into the tube  $\alpha$ ; and if it possess any sensible absorbing power, it will destroy the previously existing equilibrium. The consequent deflection of the galvanometer, properly reduced, is the measure of the absorption.

In this way the action of eight gases and thirteen vapours have been examined, and also the action of atmospheric air.

Oxygen, hydrogen, nitrogen, and atmospheric air, respectively, absorb about 0.3 per cent. of the calorific rays: this is the feeblest action which has been observed.

The most energetic action is that of olefiant gas, which at the tension of one atmosphere absorbs 81 per cent. of the calorific rays. Between those extremes stand carbonic oxide, carbonic acid, nitrous oxide, and sulphuretted hydrogen.

\* The author points out the means by which a galvanic coil of any length and of any degree of fineness, and possessing no trace of magnetism, may be obtained.



Below a certain tension, which varies for the different gases, the amount of heat absorbed is exactly proportional to the density of the gas.

Above this tension the rays on which the principal absorptive energy is exerted become gradually exhausted, so that every augmentation of density produces a diminished effect.

In the case of olefiant gas, for example, a unit-measure  $\frac{1}{10}$ th of a cubic inch in capacity being made use of; for a series of fifteen such measures the absorption was exactly proportional to the quantity of gas; subsequently the ratio of the successive absorptions approached gradually to a ratio of equality. The absorption produced by a single measure of olefiant gas of the above volume moved the index of the galvanometer through an angle of 2.2 degrees, the tension of the gas being only  $\frac{1}{11000}$ th of an atmosphere.

In the case of vapours, the most energetic is that of sulphuric ether; the least energetic is that of bisulphide of carbon. Comparing small volumes at equal tensions, the absorptive energy of sulphuric ether vapour is ten times that of olefiant gas, and ten thousand times that of oxygen, hydrogen, nitrogen, or atmospheric air.

On a fair November day the aqueous vapour in the atmosphere produced fifteen times the absorption of the true air of the atmosphere. It is on rays emanating from a source of comparatively low temperature that this great absorptive energy is exerted; hence the aqueous vapour of the atmosphere must act powerfully in intercepting terrestrial radiation; its changes in quantity would produce corresponding changes of climate. Subsequent researches must decide whether this *vera causa* is competent to account for the climatal changes which geologic researches reveal.

Oxygen obtained from the electrolysis of water exerted four times the absorptive energy of the same substance when caused to pass through iodide of potassium; the greater action being due to the presence of ozone.

The radiative power of gases was examined by causing them to pass over a heated sphere of metal, and ascend in a column in front of the thermo-electric pile; various precautions were taken, which are fully described in the memoir. It was found that the order of radiation was exactly that of absorption; that any atom or molecule which is capable of accepting motion from agitated ether, is capable

in precisely the same degree, of imparting motion to still ether. Films of gas on surfaces of polished metal are shown to act like coats of varnish.

The author has appended a theoretic chapter to his memoir, in which he investigates the physical connexion of radiation, absorption, and conduction. In the foregoing experiments we have dealt with *free* atoms and molecules, and thus fixed upon them individually the responsibility of the effects observed. The effects are thus detached from considerations of cohesion and aggregation which suggest themselves in the case of liquids and solids.

He points out that the reciprocity of absorption and radiation is a necessary mechanical consequence of the theory of an ether.

But why is one molecule competent to stop or generate a calorific flux so much more powerfully than another? The experiments point as follows:—The elementary gases which have been examined all exhibit extremely feeble powers both of absorption and radiation in comparison with the compound ones. In the former case we have oscillating atoms, in the latter oscillating systems of atoms. Uniting the atomic theory with the conception of an ether, it follows that the *compound* molecule which furnishes *points d'appui* to the ether must be capable of accepting and generating motion in a far greater degree than the single atom, which we may figure to our minds as an oscillating sphere. Thus oxygen and hydrogen, which taken separately or mixed mechanically, produce a scarcely sensible effect, when united chemically to form oscillating systems, as in aqueous vapour, produce a powerful effect. Thus also nitrogen and hydrogen, which, when separate or mixed, produce but little action, when combined to form ammonia, produce a great action. So also nitrogen and oxygen, which, as air, are feeble absorbers and radiators, when united to oscillating systems, as in nitrous oxide, are very powerful in both capacities. Comparing small volumes at equal tensions, the action of nitrous oxide is 250 times that of air; a fact, which perhaps furnishes a stronger presumption than any previously existing, that air is a *mixture*, and not a *compound*. Carbonic oxide is about 100 times as powerful as its constituent oxygen; carbonic acid 150 times as powerful; while olefiant gas, as already remarked, is 1000 times as powerful as its constituent hydrogen. In the case of the hydrocarbon vapours,

where the atomic groups attain a higher degree of complexity, the action is even greater than that of olefiant gas.

The author also refers to the experiments and observations of Niépce, Angström, Foucault ; but more especially to the admirable researches of Kirchhoff and Bunsen, as regards the influence of the period of oscillation on the rate of absorption. He points out how the grouping of atoms to systems in a resisting medium must tend to make their periods of oscillation longer, and thus bring them into isochronism with the periods of the obscure radiations made use of in the experiments.

With regard to conduction, the author would illustrate his views by reference to two substances—rock-salt and alum. He was once surprised to observe the great length of time required by a heated mass of rock-salt to cool ; but this was explained by the experiments of Mr. Balfour Stewart, who shows that rock-salt is an exceedingly feeble radiator. The meaning of this is, that the molecules of the salt glide through the ether with small loss of *vis viva*. But the ease of motion which they are thus proved to enjoy must facilitate their mutual collision. The motion of the molecules, instead of being expended on the ether between them, and then communicated in part to the ether external to the mass, is transferred freely from particle to particle ; or, in other words, is freely conducted. This *a priori* conclusion is completely verified by the author's experiments, which prove rock-salt to be an excellent conductor. It is quite the reverse with alum. Mr. Balfour Stewart's experiments prove it to be an excellent radiator, and the author's experiments show it to be an extremely bad conductor. Thus it imparts with ease its motion to the ether, but finds difficulty in transferring it from particle to particle. Its molecules are, in fact, so constituted, that when one of them approaches its neighbour, a swell is produced in the intervening ether ; this motion is immediately communicated to the ether outside, and is thus lost for the purposes of conduction. The lateral waste prevents the motion from penetrating the alum to any great extent, and hence it is pronounced a bad conductor. These considerations are dwelt upon more fully in the memoir of which this is an abstract ; and they seem to reduce the phenomena of absorption, radiation, and conduction to the simplest mechanical principles.

*February 14, 1861.*

Major-General SABINE, R.A., Treasurer and Vice-President,  
in the Chair.

The following communication was read :—

“ On Magnetic Storms and Earth-Currents.” By CHARLES V.  
WALKER, Esq., F.R.S., F.R.A.S. Received Jan. 31, 1861.

(Abstract.)

The author first refers to the movements of telegraph needles, due to causes external to the apparatus itself, which were noticed very soon after the first electric telegraphs were erected. In illustration, he gives some extracts from his diary at various dates in the year 1847; and a copy of a General Order which he issued on the 25th of October in that year, calling upon the telegraph clerks under his charge in the south-eastern district of England to take notes of these phenomena, and forward them to his office. The telegraph in use there then, as well as now, is Cooke and Wheatstone's needle instrument, having one or two vertical galvanometers.

He makes some extracts from his ‘Electric Telegraph Manipulation,’ published in 1850, showing that the impression expressed in the extracts from his diary, of a connexion between auroral manifestations and the phenomena in question, was confirmed. And he refers back to the published account of ‘The daily Observations of Magnetometers at the Royal Observatory, Greenwich, in the year 1847,’ and extracts from it the reports made of the behaviour of the magnetometers on the days cited, showing that they were very much disturbed.

The author describes the time in question as a period of great disturbance; so much so, that in the year 1848 he was constrained to adopt a device by means of which the telegraph communication might be carried on, notwithstanding the presence of these foreign influences in the wires. But his plans were hardly matured and in operation before the cause disappeared; the disturbances almost entirely ceased. The years 1847 and 1848 had been periods of great activity. The year 1849 was a period almost of inaction; and this

continued, a circumstance which, although it caused at the time some surprise, tended to withdraw attention from the question.

A paper by Mr. W. H. Barlow is then referred to, which was read before the Royal Society on May 25, 1848, and subsequently published in the 'Philosophical Transactions;' and in which a very interesting set of observations are given, made at Derby in May 1848, upon various lines of telegraph centring in that city. The relative bearing of the terminal stations, and not the route of the wires, is at the basis of these observations; and it is considered that the wires conveyed a portion of a terrestrial current of electricity, which current appeared to travel between S.  $28^{\circ}$  W. and S.  $75^{\circ}$  W.

The author goes on to state that, after a comparatively long lull, with only here and there a sign of moderate activity, attention was again called to these earth-currents about the year 1856; and more definite instructions were issued for observations to be made, and returns sent in, which has led to a large accumulation of observations, commencing early in 1857. It is not his purpose to discuss the mass of observations, nor to extract from them the dates of great disturbance; for this would only go to show the general relation between earth-current, magnetic disturbance, and aurora, which is already well established.

The most notable period of disturbance that has occurred since electric telegraphs have been in existence, was from August 27th to September 6th, 1859. It was recognized in one or more of its modes of manifestation, not only in Europe and America, but in Australia. Notices of the phenomena, collected in all parts of the world, are given by Prof. Elias Loomis in the 'American Journal of Science and Arts,' and occupy ninety-three pages. Two articles in 'Les Archives des Sciences Physiques,' by Prof. De la Rive, are also referred to. Referring to the reports in question, it was evident to the author that those who, like himself, had electric telegraphs under their control, had, to a certain extent, failed in their duty. He had, it is true, collected many observations, but they had been neither discussed nor published; and M. De la Rive had to express his regret that the returns which reached him of this great storm gave him no clue as to the direction of the currents; and for lack of this information the conclusions to which he arrived admit of reconsideration. Mr. Walker, upon learning this, took the matter up more actively.

The south-eastern district, where the observations were made, is shown by the Map which accompanies this communication, and may be regarded as bounded N. by the Thames, E. and S. by the British Channel, and W. by the other southern counties of England. Kent, Surrey, Sussex, and Berkshire, are concerned in contributing to the observations. Eighteen pairs of terminal stations have been selected; the eighteen direct lines, drawn to connect each pair of stations, make different angles with the magnetic meridian. The view taken by the author is that a flood or stream of electricity, of indefinite width, is drifting across the country, and that portions of it appear as derived currents in the telegraph wires, entering by the earth connexion at one terminus, and leaving by like means at the other. The derived current enters at the terminus nearest to the point of the horizon *from* which the main current flows, and leaves at that nearest the point *toward* which it flows.

A Table is then given of the eighteen pairs of stations, their names, the angle their joining line makes with the magnetic meridian, the direct distance from station to station, and the distance by the wire route.

The returns made of the August-September storm of 1859 are more meagre than usual, for reasons that are given; and the author explains, that on the very days when the clerks would be most valuable as observers, they are more occupied in their ordinary duties from the presence of the disturbances, which harass them in their work; and on this account he expresses himself the more indebted to those who have observed so well at such times, especially to Messrs. J. Dyke, D. Malpas, and T. Pulley.

An extract is next given from the last Report of the Astronomer Royal to the Board of Visitors, stating that he cannot extract from the returns made by telegraph clerks an idea of the phases of earth-currents, to make them comparable with those of magnetic storms. Into these views Mr. Walker enters; and while pleading guilty to not having contributed from his district any observations to the Royal Observatory, he explains that it is because he had not heretofore had the opportunity of subjecting what he had collected to anything like a fair discussion. But he quite agrees with the Astronomer Royal, in thinking that it may turn out to be desirable that observations of earth-currents should be made in a magnetic

observatory, side by side with those of the magnetometers, on wires specially erected; which wires, as appears from the sequel, need not be of any great length; for the Margate-Ramsgate group, three miles in length, is found to be very active, and action has been found on a length of 972 yards.

Reports are then given of the behaviour of the needles during the August-September storm of 1859, followed by a Table containing a series of consecutive observations which the Ramsgate clerk made from August 29th to September 2nd, and which show the duration and changes of the currents, the *general* direction in which they were moving, and their comparative values; details which were wanting to M. De la Rive when he wrote on this storm.

The author adverts to the remarkable manner in which the change from a current in one direction to a current in another is brought about, by no drift of anything like a "circular storm," nor by any kind of axial rotation; and in contrast to the calm minutes in the midst of an active period, he cites cases of activity, which are common enough in periods otherwise calm. He shows also how the value of the derived current varies with the size of the wire.

Passing from this more general view, he describes the arrangement made for forming a more definite opinion of the value of the currents collected, and how he proposed to turn to better account the next storm-days, of which some good cases followed on August 8 to 12 inclusive, and on Sept. 7, 1860, which is the last storm-day that has occurred. On the days in question a good series of observations were made, the results of which are given in Tables which accompany the communication. These Tables are discussed, and the values of the currents of either kind are given in detail in degrees and in time; and the means are deduced. Before discussing the results, the author takes occasion to call attention to the very active habit of the Margate-Ramsgate line, and to the high value of the currents collected there, although, as before stated, its length is only three miles.

On discussing the Tables, it appears that 1 current in 20 had a duration of less than half a minute. The proportion of currents above and below 5 minutes' duration was as 1 to 2.32. Of those above 20 minutes and those between 5 minutes and 20 minutes, the proportion was as 1 to 3.8. The 1-minute currents are most in number; then, in order, the 2-min., 3-min., 4-min.,  $\frac{1}{2}$ -min. and

5-min. The observations, which embraced a period of 50 hours 42 minutes, show very nearly an equal duration of N. and S. currents, the difference being  $21\frac{1}{2}$  minutes in favour of the S. currents. The mean duration of N. currents was 9.51 min. ; of S., 9.42 min. The proportion of currents exceeding  $45^\circ$  in value to those below  $45^\circ$ , was as 1 to 6.49. The  $15^\circ$  to  $20^\circ$  currents are most in number ; then follow, in order, the  $5^\circ$  to  $10^\circ$  ; then the  $10^\circ$  to  $15^\circ$ . The mean value of the N. currents was  $28^\circ.01$  ; of the S.,  $26^\circ.07$ .

Mr. Walker then refers to the opinion of M. De la Rive, that the S. currents are merely due to the secondary polarities acquired by the earth-plates, and shows that it is not in accordance with the facts here accumulated ; for, from the results given, it would be hard to say that either the N. or the S. currents exceed the other in value ; and no one thing could here be said of the N. currents, which might not, with equal truth and equal force, be said of the S. currents.

The remarks thus far have reference to a solitary telegraph group, from which the *general* drift, but not the *special* direction of the earth-currents, is gathered. The action produced might be equally due to a current running in a direction many degrees to the right or many degrees to the left of the direct line joining the two stations. In illustration of this, a series of diagrams have been prepared, in the first of which the lines of direction (referred to the magnetic meridian) of all the telegraph groups are set off ; in others, the lines alone on which observations were made at a given time, are laid down. In proportion as the two boundary lines in any case make a greater angle with each other, the absolute direction of the current is more nearly determined. Cases are given and discussed, and the conclusions progressively arrived at, in order, as lines with a greater angle occur, to give the dimensions and position of the arc of the horizon within which the resultant line of direction is situated. When observations are obtained from only one line, the resultant is known to be somewhere in one half of the horizon ; but the place is not proximately defined. Frequent observations were made on a pair of lines making an angle with each other of  $74^\circ$ . This reduced the limit within which the resultant was to be sought to  $106^\circ$ . Other observations were made on a pair of lines making an angle of  $136^\circ$ , which reduced the limit to  $42^\circ$ . And finally, a table of observations is given, which were made upon a pair of lines making an angle



of  $147^{\circ}$ , which reduces the arc within which the resultant is to be sought to  $31^{\circ}$ ; and this is the closest result that has yet come out from absolute observation. This arc is within the limits of each of the larger arcs that come out from the previous observations, and it extends from  $46^{\circ}$  to  $77^{\circ}$  E. of the magnetic north.

The currents in all the cases cited were not confined to the limiting lines, but were constantly found in one or more of the intermediate lines, and always in a direction consistent with the view taken. In the last case, where the arc is reduced to  $31^{\circ}$ , and in most of the observations tabulated, the current in the limiting lines was so strong that the author felt himself fully justified in concluding that if he had lines of telegraph beyond these limiting lines, he should have found currents: he has assumed  $10^{\circ}$  as not likely to be beyond the range; and this reduces the arc to  $11^{\circ}$ ; and knowing from experience in which limiting line the current is apt to be more active, he has divided the arc of  $11^{\circ}$  into the proportion of  $7^{\circ}$  to  $4^{\circ}$ ; and has set off a line, which may be taken to be a very close approximation to the direction in which the currents move. It falls  $63^{\circ}$  E. of magnetic N.; and by deducting from this  $21\frac{1}{4}^{\circ}$ , the value taken for magnetic declination, the direction of the earth-currents, referred to the astronomical meridian, is  $41\frac{3}{4}^{\circ}$  E. of N., or N.E. within  $3\frac{1}{4}^{\circ}$ . When the currents change in direction, it becomes S.W. within  $3\frac{1}{4}^{\circ}$ . The cases discussed go back as far as 1857; and there appears to have been the same general direction during that period. A N.W. and S.E. direction for the resultant is not known.

The author discusses, as well as he can from the few data before him, a few observations made in France and Switzerland. They are meagre in detail, but confirm the impression, which would naturally arise, that some such general direction will be found there. He also refers back to Mr. Barlow's paper, in which an arc of  $47^{\circ}$  was obtained, and within which the N.E. bearing was situated, being  $30^{\circ}$  from one limit of the arc and  $17^{\circ}$  from the other; showing that from the year 1848 to the present time no great change in azimuth has taken place.

A few remarks follow in explanation of certain specialities and anomalies in the behaviour of some of the lines in respect to their azimuths, and on the relative conductive power of the various geological strata concerned. And the author thinks it very probable that many of the currents, observed some thirty years ago in the metal-

liferous veins in Cornwall, may have been veritable earth-currents, and may have been coincident with magnetic storms.

Having determined the proximate direction of earth-currents, Mr. Walker referred to the magnetometers of an observatory merely as magnets, in order to discover whether the magnetic disturbances at a given time were in accordance with the known reaction of electric currents and magnets. The Astronomer Royal, as well as General Sabine, furnished him with the photograms from Greenwich and Kew respectively, which he required for making the comparisons. He has selected some cases of earth-currents, and has set them out in curves, side by side with the Greenwich or Kew curves, and has found a sufficient amount of coincidence to confirm the conclusion that arises, and to encourage further inquiries. He has laid down the position which the declinometer and the horizontal-force magnetometer would tend merely as magnets to assume under the influence of currents moving in the assigned direction, and has given some extracts from the Greenwich observations in support of the views he takes.

The author has made no attempt to trace the origin of the currents in question. He simply takes them as he finds them, and endeavours to arrange them in some degree of order; and he touches very lightly upon terrestrial magnetism itself. He considers that, "although we are considerably in the dark as to the forms of force in operation to make up the *whole* of the causes concerned in magnetic disturbance, we are yet quite certain that the current form of force is at least in *part* concerned." And he adds, "We can collect this force and measure it, and deal with it independently. We can receive the results and record them photographically as foreshadowed by the Astronomer Royal, side by side with those of the magnetometers. And doubtless should such combined results come at any future day under discussion—and more so, should they pass into the hands of General Sabine,—a method would be devised of eliminating the value due to these known causes, that is, due to earth-currents absolutely collected, and thereby rendering the value thus corrected more manageable; and we might get one step nearer towards penetrating into the more recondite causes of the earth's magnetism and its variations." And this cannot be accomplished until Mr. Airy's suggestion, of including the earth-currents in the observations of a magnetic observatory, is realized.

*February 21, 1861.*

Major-General SABINE, R.A., Treasurer and Vice-President,  
in the Chair.

The following communications were read :—

- I. "On Terephthalic Acid and its derivatives." By WARREN DE LA RUE, Ph.D., F.R.S. &c., and HUGO MÜLLER, Ph.D., F.C.S. Received February 7, 1861.

Whilst pursuing our investigation of Burmese naphtha, an abstract of which we have already communicated to the Society, we noticed, among the products of the action of nitric acid on certain liquid hydrocarbons contained in Rangoon tar, an acid of peculiar properties. A very lengthened investigation of this acid and its derivatives we are about bringing to a close; but as the drawing up of this account will necessarily occupy a considerable time, we have thought it desirable to send a short abstract of the chief results we have obtained, with the view of its appearing in the 'Proceedings' of the Society.

M. Caillot, about fifteen years ago, obtained a peculiar acid among the products of the action of dilute nitric acid on oil of turpentine, to which he gave the name of Terephthalic acid, on account of its generation from oil of turpentine and its isomerism with phthalic acid. M. Caillot's account of his new acid was so brief and incomplete, that, although we recognized many points of resemblance between it and the acid we had obtained from Burmese naphtha, we were compelled to repeat his experiments on oil of turpentine before we could fix with certainty the identity of the two products. In the course of these experiments, in which that identity was fully established, we noticed some interesting features in the compounds of the acid and the derivatives we discovered; more especially the relation of terephthalic acid to the well-known aromatic series,—a relation precisely analogous to that which succinic acid bears to the fatty acids. The close relation which exists between terephthalic acid and benzoic acid is most strikingly manifested in the great number of derivatives which are obtained from the former; indeed, nearly all of the most characteristic benzoyl-

compounds have their analogues amongst the derivatives of terephthalic acid. Terephthalic acid being a bibasic acid, maintains its character throughout its various transformations, and it is this fact which claims particular interest.

Terephthalic acid, as well as its derivatives, forms the first term of a new series of well-characterized bodies, and may, as such, be considered the prototype of a great number of compounds still unknown.

Without dwelling at present on the tedious process by which terephthalic acid is produced, we may mention that it is obtainable from various sources. We have, for instance, found that it is invariably formed, in a relatively small proportion, when toluyllic acid is prepared from cymol; it is also formed when cymol is treated with fuming nitric acid for the purpose of preparing nitrotoluyllic acid. It is important to mention, that whether the cymol be prepared from oil of cumín or from camphor, the result is the same.

Subsequently, we found that insolinic acid, which was described some years ago by Hofmann as a new acid of the formula  $C^6 H^6 O^4$ , is in reality terephthalic acid. The formation of this acid from oil of cumín or cumínic aldehyde by the action of chromic acid on these substances, turned out to be the most ready method of preparing terephthalic acid; and the principal part of our experiments were made with terephthalic acid which had been obtained from oil of cumín by this process.

Terephthalic acid being isomeric with phthalic acid, has the formula  $C^6 H^6 O^4$  (Carbon=12, Oxygen=16), as already known. When pure, it forms a white opaque powder; but if thrown down from a boiling dilute alkaline solution, it may be obtained in a crystalline state. When collected on a filter, these crystals dry in paper-like masses of a silky lustre. Terephthalic acid is not perceptibly soluble in ether, chloroform, acetic acid, water, or the other usual solvents. Concentrated sulphuric acid dissolves it to a considerable extent, especially when warm, without the formation of sulpho-terephthalic acid, and the acid separates unchanged on the addition of water. On heating, terephthalic acid sublimes without previously fusing. The sublimate, which is indistinctly crystalline, has the same composition and properties as the original acid, and therefore, unlike other bibasic acids, terephthalic acid cannot be converted into an anhydrite by merely heating it. Terephthalic

acid exhibits a remarkable deportment with regard to its salts; for although bibasic, there appear to exist no double salts; and even acid salts are only prepared with the greatest difficulty.

The alkaline terephthalates are all very soluble in water, but are insoluble in alcohol. The potassium, sodium, and ammonium compounds can be obtained in well-crystallized forms. Calcium and barium salts are less soluble than the before-named, and may be obtained in small scaly crystals. The copper salt is a pale blue crystalline powder. The silver and the lead salt occur as curdy precipitates when obtained by double decomposition. The compounds of terephthalic acid with the alcohol radicals possess a particular interest, as they furnish the most direct proof of the bibasic nature of the acid. There exist neutral and acid compounds. The neutral ethers are obtained either by the action of chloride of terephthalyl on the alcohols, or by means of the iodide of the alcohol radicals and terephthalate of silver or of potassium.

The methyl-terephthalic ether,  $C^6H^4(CH^3)_2O^4$ , is the most characteristic compound, and consequently may be used to detect the existence of terephthalic acid in the presence of other acids. It forms beautiful flat prismatic crystals several inches long, which fuse at a temperature above  $100^\circ$  (Cent.), and sublime without decomposition. It is readily soluble in warm alcohol, and slightly soluble in cold alcohol.

The ethyl-terephthalic-ether forms long prismatic crystals resembling urea, and is readily soluble in cold alcohol.

The amyl-terephthalic-ether forms scaly crystals of pearly lustre, is readily soluble in alcohol, and fuses in the temperature of the hand.

Phenyl-terephthalic-ether, a white crystalline substance, fuses at above  $100^\circ$  C.

The acid compounds are generally formed in small quantities, along with the neutral ethers, by the action of the iodide of the alcohol radicals on terephthalate of silver. They are well-defined monobasic acids, and form crystallizable substances soluble in alcohol.

Nitro-terephthalic acid,  $C^6H^5(NO^2)O^4$ . This acid is formed by acting with a mixture of nitric and fuming sulphuric acid on terephthalic acid. When crystallized from certain solvents, it forms well-developed prismatic crystals of a faint yellow colour. From water, it deposits in cauliflower-like aggregations.

Nitro-terephthalic acid is readily soluble in warm alcohol and in warm water, and possesses the bibasic character of the terephthalic acid in a much higher degree. It forms well-defined crystallizable acid and neutral salts. The ethers of this acid are likewise crystallizable. They differ, however, from the terephthalic acid ethers by their greater solubility in alcohol and their depressed fusing-point.

Chloride of terephthalyl ( $C^6H^4O^2Cl^2$ ) is obtained, together with oxychloride of phosphorus (hydrochloric acid being evolved), when terephthalic acid is acted upon with pentachloride of phosphorus at a temperature of  $40^\circ$  (Cent.). Chloride of terephthalyl is a solid and beautifully crystalline substance, without odour at the ordinary temperature, but evolving, when heated, a very pungent smell like that of chloride of benzoyl, which it resembles in all its reactions. With the alcohols it forms terephthalic ethers, with ammonia an amide, and with the organic bases compound amides. Terephthalylamide,  $C^6H^4N^2O^2$ , can only be obtained by acting with chloride of terephthalyl on ammonia; it is a white amorphous substance insoluble in all solvents. Terephthalylamide, when treated with fuming nitric acid, yields nitro-terephthalic amide,  $C^6H^4(NO^2)N^2O^2$ , which crystallizes in beautiful prisms.

Terephthalamide shows a remarkable resemblance to benzamide when treated with substances capable of abstracting the elements of water. It loses two equivalents of water ( $H^2O$ ), and is converted into terephthalynitril,  $C^6H^4N^2$ . This remarkable substance is best formed by the action of anhydrous phosphoric acid on terephthalamide. It distils over in form of a liquid, which solidifies in the neck of the retort.

Terephthalynitril is colourless and without odour, and forms beautiful prismatic crystals. It is insoluble in water, readily soluble in boiling alcohol, less soluble in cold alcohol, and insoluble in benzol. When boiled with caustic alkalies, it is gradually decomposed, ammonia is given off, and terephthalic acid is reproduced.

It is obvious that terephthalynitril, like all similar substances, may be considered as a cyanogen compound, which in this instance would be the cyanide of the bibasic radical phenylen,  $C^6H^4$ , which is not yet discovered. If we could succeed in obtaining phenylen, the artificial production of terephthalic acid or an isomeris would probably be attended with little difficulty.

By acting on nitro-terephthalic acid with reducing agents, it undergoes the same change as other nitro-compounds. The product of this reaction is the oxy-terephthalamic acid, or the analogue of the glycocoll of the formula  $C^8 H^7 NO^4$ . This new member of the glycocolls is a lemon-yellow substance, crystallizing in thin prismatic, and sometimes moss-like forms. It is very slightly soluble in cold water, alcohol, ether, and chloroform. Like other substances of this kind, terephthal-glycocoll combines with bases as well as with acids. The salts formed with the bases are crystalline; they are readily soluble in water and dilute alcohol, yielding colourless solutions of most remarkable fluorescent properties, which have been investigated by Professor Stokes.

The aqueous and alcoholic solution of the pure terephthalic glycocoll shows the same properties. The compounds with acids crystallize well, and if dissolved in a large quantity of water decompose. They do not possess the fluorescence when in their acid solution.

The ether-like compounds of oxy-terephthalamic acid are obtained by acting upon the corresponding ethers of the nitro-terephthalic acid with reducing agents. The methylic ether is a beautiful crystalline substance, readily soluble in warm alcohol, but much less soluble in any of the solvents than nitro-terephthalate of methyl. The ethylic ether crystallizes in large crystals with an appearance resembling those of nitrate of uranium. The solutions of this ether possess the fluorescent property in the highest degree. Oxy-terephthalamate of methyl and ethyl combine with acids and form well-defined salts. Oxy-terephthalamic acid, as well as its ether, are readily acted upon by nitrous acid, this reaction giving rise to a number of new derivatives, which vary in their nature according to the condition in which the reaction takes place.

M. Griess has lately made us acquainted with a new class of remarkable substances which are obtained by the action of nitrous acid on a certain class of nitrogenous bodies. The several derivatives he obtained by this reaction from oxy-benzamic acid have their representatives in the bibasic terephthalyl series, and are obtained with the utmost facility. On acting with nitrous acid upon an aqueous solution of the oxy-terephthalamic acid instead of an alcoholic solution, as is employed in Griess's reaction, this substance is readily decomposed, nitrogen is given off in large quantities, and there gradually separates

a whitish substance which is oxy-terephthalic acid,  $C^8 H^4 O^4$ . This acid is a substance of great interest, and its preparation offering much less difficulty than the analogous oxy-acids of the aromatic series, it affords an opportunity of studying to a fuller extent the nature of this class of acids, especially as it may be expected that the history of this acid will throw some light on the law of polybasicity. Oxy-terephthalic acid forms beautiful crystalline salts, which are less soluble than the corresponding terephthalates. The neutral ethers are liquid.

The chloride of oxy-terephthalyl is likewise a liquid readily decomposed by water and alcohols.

- II. "Notes on the Generative Organs, and on the Formation of the Egg in the Annulosa."—Part I. By JOHN LUBBOCK, Esq., F.R.S. Received February 5, 1861.

(Abstract.)

In the present paper I have communicated some observations on the Myriapoda, on *Petrobius*, and on certain Arachnida. Among the former I have examined species belonging to the genera *Glomeris*, *Iulus*, *Polydesmus*, *Lithobius*, *Cryptops*, *Geophilus*, and *Arthrionomalus*. Through the labours of Brandt, Fabre, Newport, Stein, Treviranus, and other eminent naturalists, we are tolerably well acquainted with the anatomy of the generative organs in the Myriapods; but these observers have occupied themselves principally with the arrangement and forms of the organs, and have not paid much attention to the different stages of egg-development, nor to the relation in which the young egg stands to the surrounding tissues. This relation is indeed very curious, and seems to have been generally misunderstood. It is well known that there are in the Myriapods no long egg-tubes, as in most insects, but that each egg arises in a separate follicle. It was, however, natural to suppose that this follicle held the same position with reference to the ovary as the very similar egg-follicles of certain insects, as, for instance, of *Coccus*. This, however, is by no means the case. If we compare the ovary and egg-follicle of *Coccus* with the ovary and egg-follicle in *Glomeris*, we shall see that the egg-follicle is very much alike in both cases: the



shape of the follicle, the Purkinjean vesicle, and the vitelligenous cells being very similar ; but whereas in *Coccus* and in all insects the egg-follicle projects *from* the ovary, in *Glomeris* and the other Myriapods, so far as my observations go, the follicle projects *into* the ovary. If, therefore, we consider the ovary as consisting of an outer membrane and an inner epithelial layer, it would appear that while the egg in the Myriapods arises between these two layers, in the insects it originates on the inner side of both.

This difference appears to me to be very important, and, as will be mentioned under the head of *Iulus*, escaped the attention of our great anatomist Newport, whereby he was led to give an erroneous description of the ovary of that genus. I have chosen to compare *Glomeris* with *Coccus*, because the vitelligenous cells make the resemblance, and, at the same time, the difference between these two genera more striking. If we, however, compare with them the ovary of *Phalangium*, we shall see not only that the vitelligenous cells are absent, but that the egg-follicle differs equally from that of the insect on the one hand, and that of the Myriapod on the other. The egg-follicle projects *from* the ovary as in *Coccus*, &c. ; but, on the other hand, the Purkinjean vesicle lies on the outer side of the epithelial layer, as in *Glomeris*, and in consequence the egg-follicle, which in *Coccus* consists of both the ovarian membranes (so far as the epithelial layer can be called a membrane), and in *Glomeris* of the epithelial layer only, is in the Phalangidæ, and perhaps all the Arachnida, formed only by the outer membrane. Consequently, while in the insects the mature ovum passes into the ovary through the neck of the follicle, in Myriapods and the Arachnida it bursts through the epithelial layer, in the former at its free, and in the latter at its attached end.

If these characteristics are found eventually to hold good throughout the Myriapods and Arachnida, the differences thus shown to exist between these groups will be of great interest ; but it is too early to generalize on the subject with much confidence. Moreover, it often happens that one or a few epithelial cells *are* attached in Arachnida to the inner side of the follicle-wall. This happens, however (as far as my observations go), without any regularity, and the cells thus present fulfil perhaps no important function in the formation of the egg. The Crustacea appear to differ from the three preceding groups in the fact that their eggs do not possess separate follicles.

In the more advanced eggs of *Glomeris*, the macula of Wagner is double, one being much smaller than the other.

The ovary of *Iulus* has been described by Newport; the specimens which he examined had, however, been submitted to the action of spirits of wine, and were therefore altogether unsuitable for histological examination. The egg-follicle did not contain any large vitelligenous bodies like those of *Glomeris*, nor could I see in it a vitelline vesicle like that of *Arthronomalus*; but it very generally contained, when the yelk was beginning to darken, a patch resembling that which appears to result from the small vitelline vesicle of *Lithobius*. In both these genera the eggs in all stages of development are mixed together without any regular arrangement. The macula appears to be generally double, as in *Glomeris*. In the genus *Polydesmus*, however, where, as in all Chilognaths, the eggs are arranged on a double stroma, situated at the lower side of the ovary, the smaller eggs are on the central parts of the stroma, and the more developed ones lie towards the sides. In appearance the ova offer a great contrast to those of *Iulus*, and particularly of *Glomeris*, since the margin of the yelk has all the appearance of being bounded by a distinct membrane, though I am not convinced that this is really the case. I have not yet made out in *Polydesmus* anything corresponding to the "patch" of *Iulus*. The macula appears to be always single. It often contains vacuoles.

The yelk of mature eggs consists,—1st, of globules about  $\frac{1}{8000}$ th of an inch in diameter; 2ndly, of greenish bodies about  $\frac{1}{4000}$ th in diameter; and 3rdly, of a clear, more or less viscid substance. By the month of October most of the females have laid their first lot of eggs; after which another set immediately begin to be developed. M. Fabre does not appear to have seen the spermatozoa of *Polydesmus*. They are small, elliptical bodies about  $\frac{1}{8000}$ th of an inch in length, and containing a bright nucleus. M. Stein appears to be the only naturalist who has hitherto described them from personal observation, but he does not allude to the nucleus.

In *Lithobius*, *Cryptops*, *Geophilus*, and *Arthronomalus*, the eggs form a single series; large ones and small ones lie next to one another without any arrangement. In the youngest Purkinjean vesicles of *Lithobius* there are about seven nuclei, each  $\frac{1}{8500}$ th of an inch, or thereabouts, in diameter. As the eggs increase in size these maculæ

appear to subdivide, at any rate they become continually smaller and more numerous. This is very remarkable, nothing like it taking place in the other genera. When the egg has attained a certain size, but before it has begun to darken, a small vesicle, about one-third of the size of the Purkinjean vesicle, may generally be seen in it. This vesicle is probably homologous with the vitelline vesicle of *Arthronomalus*, &c. It soon disappears, and is replaced by a "patch" resembling that of *Iulus*\*. The eggs of *Cryptops* are narrow. The macula differs from that of *Lithobius*, and agrees rather with that of *Glomeris*, in consisting of two unequal separate bodies. To the description of the spermatophores given by M. Fabre, I have nothing of importance to add. The spermathecae of the females examined by me contained only filiform spermatozoa. The form of the eggs in *Arthronomalus* is spherical. The yolk-nucleus is very apparent, and has at first all the appearance of a vesicle. Globules gradually appear in it, and it loses its distinct outline.

The development of the eggs in *Geophilus* is much like that of *Arthronomalus*. The principal peculiarity observed was in the structure of the Purkinjean vesicle, which appeared to consist of two different substances, one surrounding the other. The inner substance was generally produced in the form of a peninsula at one or two places, almost through the outer substance. This latter was of more regular outline, but sometimes partially followed the same outline. Occasionally even the macula was thus produced. It appeared to be that the projecting parts gradually became pinched off from the main portion, and thus formed certain irregularly rounded bodies, which could in most eggs be indistinctly discerned in the homogeneous yolk, and were probably homologous with the vitelline vesicle of *Arthronomalus*. A somewhat similar phenomenon has been described by Leuckart as occurring in the eggs of *Aphis rosæ*.

In the Phalangidæ I examined the genera *Phalangium*, *Nemastoma*, *Opilio*, and *Leiobumus*, in all of which the process of egg-development was similar, and resembled that already described as occurring in *Polydesmus*. The structure of the male generative organs has, however, been entirely misunderstood by all preceding writers. Tulk states

\* The number of labial teeth, which is a point generally used as a specific character, varies in many individuals; out of twenty-eight specimens examined, nine were unsymmetrical in this respect.

that the "testes are formed by a cluster of elongated . . . cæcal tubes;" and this opinion has been generally adopted, though from the structure and contents of these tubes it is evident that they are rather to be regarded as accessory glands. Leydig found among these tubes one longer than the rest, and described this, which is in fact the ductus ejaculatorius, as the true testis. The true testis, however, had been misunderstood, but not overlooked. It is described by Treviranus, Tulk, Leydig, and others, and being present only in the male sex, was supposed by all to be a portion of the generative organs, though they were not able to trace its connexion with the penis. It is a large V-shaped tube, which lies transversely across the abdomen; at each end it dwindles into a very fine tube, which passes, on each side, round the tracheal trunks close to the spiracles, and then the two approaching one another, unite in the centre of the body and fall into the long winding *ductus ejaculatorius*. Thus it will be seen that the generative organs of the male Phalangidæ are formed on exactly the same type as those of the females.

In *Chelifer* and *Obisium* the ovary is a simple tube, on each side of which the egg-follicles are produced. In each specimen there are two sets of eggs in very different stages of development, and it would appear that thirty or forty come to maturity at once. In the early part of the autumn I found four or five specimens, with an egg-capsule attached to the underside of the abdomen. These egg-capsules, however, only contained seventeen or eighteen eggs. Moreover, in these specimens I found neither a testis nor an ordinary ovary, but a large body consisting of about thirty follicles opening into a common cavity, and full of oil-globules. The eggs in the egg-capsule were in several cases undergoing segmentation.

The males of *Chelifer* appear to be about as numerous as the females. The testis consists of a median and two lateral tubes, which are united together by three transverse branches. It is therefore much like that of the true Scorpions. The spermatozoa in *Obisium* have a small head and a long tail, while those of *Chelifer* are oval bodies. These oval bodies may be immature forms only, but I never found any further stage of development in the spermatheca of the female.

The ovary of *Petrobius* consists of seven short egg-tubes; each of which in September contained three eggs, with more or less darkened

yolk, and about fifteen or twenty egg-germs in earlier stages. The development of the eggs reminded me much of that which takes place in the Orthoptera.

It appears to be a pretty well established fact that, at least in many animals, the Purkinjean vesicle is a modified ovarian cell, round which the yolk is deposited. On the other hand, according to some eminent naturalists, there are certain animals in which the ovarian cell becomes the egg, and its *nucleus* the Purkinjean vesicle. In this case we should have two distinct classes of eggs, the Purkinjean vesicle in one of which would be homologous with the whole egg in the other. I have, however, given in my paper the reasons which induce me to doubt whether these last observations are altogether correct. But even if we may admit that no essential difference has as yet been proved to exist in the eggs of animals, as far as regards the relations existing between the Purkinjean vesicle and the original ovarian cell, it would still seem that in the relations between the former and the yolk, two very different types of development must be recognized.

In describing the so-called "winter-ova" of *Lacinularia socialis*, Prof. Huxley says (Micr. Journal, vol. i. p. 14), "It will be observed that all these authors consider the winter-ova or ephippial ova and the ordinary ova to be essentially identical, only that the former have an outer case. The truth is that they are essentially different structures. The true ova are single cells which have undergone a special development. The ephippial ova are aggregations of cells (in fact, larger or smaller portions, sometimes the whole of the ovary), which become enveloped in a shell, and simulate true ova." This aggregation of several cells (one of them putting on the appearance and fulfilling the functions of a Purkinjean vesicle), and the whole becoming enveloped in a shell, is, however, the ordinary and only method of egg-development in many lower animals. In the Trematoda and Cestoid worms, and the greater number of the Turbellaria, the yolk and the Purkinjean vesicle are formed in two separate organs.

In *Piscicola*, according to Leydig, the mature egg contains, besides the Purkinjean vesicle and the ordinary yolk, a number of nucleated cells (Zeit. f. Wiss. Zool. 1849. part 1. pl. 10. fig. 56).

In the Mites and Spiders, in *Chelifer*, *Obisium*, the Phalangidæ, and, so far as I know, all the Arachnida, the egg arises from the metamorphosis of a single cell.

On the other hand, we find that complex eggs alone are present in vast numbers of insects, namely, in all the Lepidoptera, Diptera, Neuroptera (excluding the Libellulidæ and allied genera), Hymenoptera, Hemiptera, Homoptera, and Coleoptera. We are as yet ignorant of the mode of egg-development in the Thripidae and the Strepsiptera; nor does it seem quite clear whether the development of the pseudovum in *Aphis* can be referred to the complex type. It would, however, appear from the statements of Leydig, Huxley, and Leuckart, that in the opinion of these three eminent naturalists the pseudovum is a derivation of a single ovarian cell, and differs therefore in this respect from the ovum of the impregnated female.

We know little as yet about the early stages of egg-formation in the Crustacea, but it would appear that the simple mode prevails generally throughout this class, with the exception of the Daphnidæ.

As regards the Rotatoria, the so-called winter eggs have been observed in *Hydatina*, *Brachionus*, and *Notommata*, as well as in *Lacinularia*; and we may probably conclude that in these and other allied genera the development of these eggs is on the same type; while "summer eggs" are formed from one cell. Among the Myriapoda, the eggs of *Lithobius*, *Cryptops*, *Geophilus*, *Arthrone-malus*, *Polydesmus*, and *Iulus* are simple, the vitelline vesicles occurring in some of them being probably homologous with the yolk nucleus of Spiders.

*Glomeris*, however, offers apparently an exception to the rule so general among the Myriapoda, as the large rounded bodies present in the egg-capsule are probably homologous with the vitelligenous cells of insects.

In excluding the ephippial ova of *Lacinularia* from the category of true eggs, Prof. Huxley was influenced to a certain extent by the supposition that they are fertile without impregnation, and are therefore "not ova at all in the proper sense, but peculiar buds." According to Stein, however, the reverse is probably the case, and the summer eggs are agamic, while the winter eggs require to be fertilized. This is also in accordance with the case of *Daphnia*. In this genus, as in Rotatoria, the "summer eggs" are agamic, but it has not yet been conclusively proved that the "winter eggs" of either require impregnation. However this may be, the development of the eggs of insects sufficiently proves that eggs composed of several ovarian

cells, like those which are unicellular, generally are incapable of development without impregnation. But no one can deny the name of true eggs to the ova of Butterflies, &c.; and we cannot therefore class as "false eggs" those which arise from more than one cell. Perhaps it would be better to distinguish the two classes as "compound," and "simple" or unicellular. The names we may adopt are, however, of less importance than the establishment of the fact that throughout the Annulosa there are two sorts of eggs, which are of an essentially different structure, and cannot therefore, strictly speaking, be regarded as homologous with one another.

*February 28, 1861.*

Major-General Sabine, R.A., Treasurer and Vice-President,  
in the Chair.

The following communications were read.

- I. "Tables of the Weights of the Human Body and the Internal Organs in the Sane and Insane of both Sexes at various Ages." By ROBERT BOYD, M.D., F.R.C.P., Physician to the County Somerset Lunatic Asylum. Communicated by Dr. SHARPEY, Sec. R.S. Received February 7, 1861.

(Abstract.)

These Tables have been compiled from notes of 2086 examinations made at St. Marylebone Infirmary between 1839 and 1847, and of 528 examinations in cases of insanity made at the Somerset Lunatic Asylum, between 1848 and the end of December 1860, comprehending, in all, a period of twenty-one years. The Tables are submitted with the hope that they may aid in forming a standard of the weight of the human organs at different stages of life from early infancy to old age. The cases are distributed under eighteen periods of life, and the Tables show the height and weight of the body, and the weight of the encephalon and its several parts, the right and left lung, the heart and all the abdominal organs; giving the maximum, minimum and average in each period. In the second series of cases, namely those from the lunatic asylum, the measurement of the head, and the weight of the spinal cord are also stated.

The assigned causes of death are given in the margin, also the deviations in weight of the lungs, heart and liver, occasioned by disease.

The following general results may be deduced from the first series of cases, namely, those occurring among the Poor of the Parish of Marylebone.

With few exceptions the body and internal organs arrived at their full size in both sexes between twenty and thirty years. In children especially, the body was attenuated from disease: for example, one scrofulous female child, aged three years, weighed only 8½ lbs., and the numbers about that age were insufficient to counterbalance the effect of such cases on the mean result, and form a standard of comparison for children of the same age under more favourable circumstances. The average weight of the males was greatest at from 70 to 80 years, which is to be accounted for by the large proportion cut off at earlier periods by pulmonary phthisis. The mean weight of the male brain was, at all periods, above that of the female, which was the probable cause of the large number of still-born male infants as compared with females, 51 to 32, and the necessity of resorting to craniotomy in five instances of the former only. The highest average weight of the brain in both sexes was from 14 to 20 years; the next highest was in the males from 30 to 40, and in the females from 20 to 30 years; but it will be observed that the cases were much fewer in number in these than in other later periods. The weight of the lungs was so much, and so frequently increased by disease, that healthy lungs were exceptions: it therefore appeared advisable to introduce in the margin their weights in various states, also the weights of the heart and liver, which were subject to great variations. The "Thymus gland," an organ which disappears with infancy, was so large in the fœtus in fourteen cases, that it appeared to have formed a fatal impediment to respiration. The abdominal organs were generally heavier in the male than in the female; the spleen in both was subject to considerable variations in size, and the mean weight of the left kidney was generally found greater than that of the right.

The general results obtained from a review of the second series of cases are stated by the author as follows:—

The average height of the adult male varied from 67·8 to 65



inches, of the female from 63·2 to 61·6 inches; while the mean weight of the former varied from 112·12 to 91·5 lbs., and of the latter from 95·2 to 76·9 lbs., showing a preponderance of the insane male of 6 lbs., and of the insane female of 8 lbs., as compared with the sane adults dying at the same period of life.

The average weight of the right cerebral hemisphere varied in the males from 20·89 oz. to 18·97 oz., and in the females from 19·21 oz. to 17·20 oz.; the left varied in the males from 21·05 oz. to 18·62 oz., and from 19·51 oz. to 17·39 oz. in the females. It is a singular fact, confirmed by the examination of nearly 200 cases at St. Marylebone, in which the hemispheres were weighed separately, that almost invariably the average weight of the left exceeded that of the right by at least the eighth of an ounce. In the *Med. Chir. Trans.* vol. xxxix., several cases of inequalities of the cerebral hemispheres which came under my notice are given. The average weight of the cerebellum varied in the males from 5·42 oz. to 5·06 oz., and from 5 to 4·74 oz. in the females; that of the pons Varolii and medulla in the male from 1·15 oz. to 1·02 oz., and from 1·05 oz. to ·95 oz. in the females; and that of the encephalon in males from 48·17 oz. to 43·87 oz., and in females from 44·55 oz. to 40·55 oz.; in the sane, at the same period of life, the average varied in the male from 48·2 oz. to 45·34 oz., and in the female from 43·7 oz. to 39·77 oz.

The general average weight of the lungs is shown in the Table, and the exceptions in the margin. The average weight of the heart did not reach its maximum until an advanced period of life.

In the abdominal organs nothing was observed differing essentially from those in Table No. 1.

## II. "On the Electric Conducting Power of Copper and its Alloys." By A. MATTHIESSEN, Ph.D. Received Feb. 14, 1861. Communicated by Professor W. THOMSON, F.R.S.

The difference in the numerical results obtained by Prof. W. Thomson (*Proceedings of Roy. Soc.* 1859, x. p. 300), and those by Dr. Holzmänn and myself (*Phil. Trans.* 1860), on the conducting power of copper and its alloys, made it somewhat necessary to re-investigate the subject, in order to ascertain the cause of these differences. For this purpose Professor Thomson kindly placed at my

disposal all his alloys; and in the following Table I will give the results of the analyses and redeterminations of the conducting power of his set. The wires were in some cases very faulty, so that I was obliged to draw them finer; others drew so badly, that the values obtained could not very well agree with those already published. After having measured their resistances, I sent them back to Prof. Thomson for redetermination. Table I. gives the results so obtained, taking the alloy containing 99·75 copper and ·25 silver=100; and Table II. the values found for some specimens of pure copper:—

TABLE I.

Composition according to Messrs. Johnson and Matthey.	Analyses of Alloy.	Specific Conductivity.		
		Values found by Professor Thomson.		Values found by myself.
		Published Values.	Redetermined Values.	
Copper 99·75..	Silver 0·24 p. c.	} 100	100·1	100·37 at 17°*
Silver 0·25....	traces of iron Suboxide of copper		99·9	99·73 at 17°
Copper 99·87..	Silver 0·13 p. c.	} 100·7	95·8	95·44 at 17°·8
Silver 0·13....	traces of iron Suboxide of copper		95·8	94·58 at 17°·8
Copper 99·75..	Lead 0·2 per cent.	} 103·9	102·7	102·80 at 17°
Lead 0·25 ....	traces of iron Suboxide of copper		103·1	102·62 at 17°·6
Copper 99·75..	Tin 0·23 per cent.	} 94·6	100·7	99·89 at 18°
Tin 0·25 .....	traces of iron Suboxide of copper		101·0	98·27 at 16°·4
Copper 99·87..	Tin 0·07 per cent.	} 96·0	97·7	97·79 at 18°
Tin 0·13 .....	traces of iron. Suboxide of copper		98·5	97·62 at 18°
Copper 99·2 ..	Zinc, with traces of	} 90·2	91·3	94·71 at 15°·4
Zinc 0·8 .....	iron, 1·06 per cent.		88·5	90·67 at 15°·6
Copper 98·6 ..	Zinc, with traces of	} 74·7	81·1	81·15 at 16°·8
Zinc 1·4 .....	iron, 1·47 per cent.		80·1	80·13 at 17°·7
Copper 98·2 ..	Zinc, with traces of	} ..	77·9	77·8 at 16°·4
Zinc 1·8 .....	iron, 1·75 percent.		78·5	78·0 at 17°
Pure copper ..	Contained suboxide of copper	} 100	98·6	
Copper 99·87..	.. ..	} 104·7		
Lead 0·13 ....	.. ..			

\* Compared with a hard-drawn gold-silver wire of equal diameter and length, whose conducting power is equal at 0° C. to 100, these values would be 603·7 and 600·5. (See my paper "On an Alloy which may be used as a Standard of Electrical Resistance," Phil. Mag. Feb. 1861.)

TABLE II.

Composition according to Messrs. Johnson and Matthey.	Analyses of Alloy.	Specific Conductivity.		
		Values found by Professor Thomson.		Values found by myself.
		Published Values.	Redetermined Values.	
Pure copper electrolyte from Messrs. De la Rue .....	.. ..	.. ..	107 at 9°	107·2 at 10°
Ditto from Messrs. Elkington and Co. ..	All not fused. .. ..	.. ..	107·5 at 12°	105·9 at 10°·5
Ditto from Mr. Matthews ....	.. ..	.. ..	108·7 at 12°	106·9 at 14°
Ditto, my own...	.. ..	.. ..	107·7 at 12°	108·1 at 10°

All the above wires were hard-drawn. On looking at the above, we find that *pure copper conducts better than any of the alloys.*

With regard to the analyses, the quantity of each specimen was so small that they could not be checked by repetition; they, however, approach very closely to the composition assigned to them by Messrs. Johnson and Matthey (with the exception of the suboxide). The traces of iron will be due to the draw-plates. I will now make a few remarks on the above results.

I. That copper containing 0·25 per cent. of silver conducts better than that with 0·13 per cent., may be explained by assuming that the first contains less suboxide than the second; for it is very possible that copper containing silver will not absorb suboxide so readily as the purer metal. It must also be borne in mind that the copper employed for making these alloys was in all probability simply electrolyte copper (not fused), and that the suboxide therefore was absorbed during the process of fusing the two metals together. This assumption explains how it is that the alloys contain almost the same amount of impurity as was originally alloyed with the copper; for had the copper employed contained suboxide, we should have expected to have found greater differences in the cases of the tin, lead, and zinc alloys, as some portion of those metals would have been oxidized at the expense of part of the suboxide of copper, and escaped as oxide to the surface of the melted metal.

II. That copper containing 0·25 per cent. tin conducts better than that containing 0·13 per cent., may also be explained by assuming that they absorbed different amounts of suboxide during the process of fusion; for although tin, in presence of suboxide of copper, would be oxidized, yet copper retains the suboxide so tenaciously, that portions will always remain with the copper.

III. The fact that the conducting powers of the alloy of copper containing 0·25 per cent. lead approaches the nearest of those which I analysed to that of pure copper, is, in my opinion, a proof that the alloy is probably a mechanical mixture of copper, traces of lead, and enough suboxide to allow its being drawn into wire, and not a solution of lead in copper; otherwise a much lower conducting power ought to have been found; for, according to my own experiments, it requires twice as many volumes per cent. of lead as of tin to reduce (within certain limits) the conducting power of a metal (bismuth, silver, &c., and copper, for it belongs to the same class) to the same value: thus, to reduce the conducting power of silver to 67, it would require 0·9 volume per cent. of lead, or about 0·4 volume per cent. of tin; to reduce it to 47·6, it would require 1·4 volume per cent. of lead, or 0·7 volume per cent. of tin, &c. (Phil. Trans. 1860). Dr. Holzmänn and myself repeatedly tried to draw pure copper alloyed with 0·25 of lead without success; the alloy was perfectly rotten, which also seems to indicate a mechanical mixture.

IV. It is curious that the zinc alloys contained no suboxide.

The reason, therefore, of the difference in our results is simply that Messrs. Johnson and Matthey did not use those precautions in fusing their copper and its alloys which are necessary to ensure good results; for had they taken those precautions to prevent the absorption of oxygen by their copper and its alloys which Dr. Holzmänn and myself did, and which are fully described in our paper on the subject (Phil. Trans. 1860), the lead-copper alloys which they supplied to Prof. Thomson would not have been superior in conductive quality to the unalloyed electrotpe copper; and he would have been led to the same conclusion as that which Dr. Holzmänn and myself arrived at, namely, *that there are no alloys of copper which conduct better than pure copper*. Professor Thomson, in his paper, states that it is his opinion that the differences he observed in the conducting powers of his alloys must depend upon very small ad-

mixtures of probably non-metallic impurities. This conclusion is completely borne out by the above, as well as by the investigation carried out by myself in conjunction with Dr. Holzmänn.

The results obtained by Prof. Thomson show the marked influence of traces of foreign metals on the conducting power of pure copper,—which is fully confirmed in our research on the same subject. Professor Thomson's best-conducting alloy has a much higher conducting power than those found by some experimenters for electrottype copper ; but it must be remembered that in all probability the copper had been previously fused, and therefore contained suboxide of copper. The fact that electrottype copper may be drawn without having been previously fused is, I believe, generally not known ; Professor Buff of Giessen first drew my attention to it, and stated that he always obtained high values for the conducting powers of electrottype copper when drawn without previous fusion. I can confirm this statement, having tested a great many specimens, and found the values in all cases nearly the same.

*March 7, 1861.*

**Major-General SABINE, R.A., Treasurer and Vice-President,  
in the Chair.**

In accordance with the Statutes, the Secretary read the names of the Candidates for election into the Society as follows :—

Somerville Scott Alison, M.D.  
Alexander Armstrong, M.D.  
Lucas Barrett, Esq.  
Charles Spence Bate, Esq.  
Henry Foster Baxter, Esq.  
Alexander Theophilus Blakely,  
Capt. R.A.  
Sir Charles Tilstone Bright.  
William White Cooper, Esq.  
Joseph Cubitt, Esq.  
Richard Cull, Esq.  
Henry Duncan Preston Cuning-  
ham, Esq.

Heinrich Debus, Esq.  
Campbell De Morgan, Esq.  
Thomas Rowe Edmonds, Esq.  
Alexander John Ellis, Esq.  
James Fergusson, Esq.  
John Braxton Hicks, M.D.  
Thomas A. Hirst, Esq.  
The Rev. Abraham Hume, D.C.L.  
Henry Letheby, B.M.  
Waller Augustus Lewis, Esq.  
A. Matthiessen, Esq.  
Henry Maudslay, Esq.  
J. Clerk Maxwell, Esq.

Charles Watkins Merrifield, Esq.	Philip Lutley Sclater, Esq.
Gavin Milroy, M.D.	Charles Frederick Alexander
Rev. Walter Mitchell.	Shadwell, Capt. R.N.
Ferdinand Mueller, M.D.	Maxwell Simpson, Esq.
William Newmarch, Esq.	Henry John Stephen Smith, Esq.
Laurence Oliphant, Esq.	William Stokes, M.D.
Edmund Alexander Parkes, M.D.	George Johnstone Stoney, Esq.
Frederick William Pavy, M.D.	Alexander Strange, Major 7th
George Peacock, Esq.	Madras Cavalry.
John George Perry, Esq.	Thomas Tate, Esq.
Bedford Clapperton T. Pim.	Henry Ward, Capt. R.E.
William Pole, Esq.	C. Greville Williams, Esq.
Henry E. Roscoe, Esq.	James Young, Esq.

The following communications were read :—

- I. "On the Porism of the in-and-circumscribed Polygon."  
By ARTHUR CAYLEY, Esq., F.R.S. Received February  
20, 1861.

(Abstract.)

The Porism referred to is as follows, viz. two conics may be so related to each other, that a polygon may be inscribed in the one, and circumscribed about the other conic, in such manner that any point whatever of the circumscribing conic may be taken for a vertex of the polygon. I gave in the year 1853, in the *Philosophical Magazine*\*, a general formula for the relation between the two conics, viz. if  $U=0$  is the equation of the inscribed conic,  $V=0$  that of the circumscribed conic, and if disc  $(U+\xi V)$ , where  $\xi$  is an arbitrary multiplier, denotes the discriminant of  $U+\xi V$  in regard to

\* See the papers—"On the Geometrical Representation of the Integral  $\int dx \div \sqrt{(x+a)(x+b)(x+c)}$ ," *Phil. Mag.* April 1853.

"Note on the Porism of the in-and-circumscribed Polygon," *Phil. Mag.* August 1853.

"Correction of two Theorems relating to the Porism of the in-and-circumscribed Polygon," *Phil. Mag.* November 1853.

"Developments of the Porism of the in-and-circumscribed Polygon," *Phil. Mag.* May 1854.

the coordinates  $(x, y, z)$  (such discriminant being of course a cubic function in regard to  $\xi$ , and also in regard to the coefficients of the two conics  $U, V$ , jointly), then if we write

$$\sqrt{\text{disc}^2 (U + \xi V)} = A + B\xi + C\xi^2 + D\xi^3 + E\xi^4 + F\xi^5 + G\xi^6 + \&c.,$$

the relations for the cases of the triangle, pentagon, heptagon, &c. are

$$C =, \begin{vmatrix} C, D \\ D, E \end{vmatrix} = 0, \begin{vmatrix} C, D, E \\ D, E, F \\ E, F, G \end{vmatrix} = 0, \&c.$$

respectively, while those in the cases of the quadrangle, hexagon, octagon, &c. are

$$D = 0, \begin{vmatrix} D, E \\ E, F \end{vmatrix} = 0, \begin{vmatrix} D, E, F \\ E, F, G \\ F, G, H \end{vmatrix} = 0, \&c.$$

respectively. The demonstration of this fundamental theorem is for greater completeness here reproduced; but the chief object of the memoir is to direct attention to a curious analytical theorem, which is an easy *à priori* consequence of the Porism, and to obtain the relations for the several polygons up to the enneagon, in a new and simple form, which puts in evidence *à posteriori* for these cases, the analytical theorem just referred to. The analytical theorem rests upon the following considerations:—the relation for a hexagon ought to include that for a triangle; in fact a triangle with its sides in order twice over is a form of hexagon; the condition for an octagon should in like manner include that for a quadrangle; and so in other cases. Let the cubic function  $\text{disc}^2 (U + \xi V)$  be represented by  $1 + \beta\xi + \gamma\xi^2 + \delta\xi^3$ , the coefficients  $A, B, C, D, E, \&c.$  are functions of  $\beta, \gamma, \delta$ . Write

$$C = (3),$$

$$D = (4),$$

$$\begin{vmatrix} C, D \\ D, E \end{vmatrix} = (5),$$

$$\begin{vmatrix} D, E \\ E, F \end{vmatrix} = (6),$$

$$\begin{vmatrix} C, D, E \\ D, E, F \\ E, F, G \end{vmatrix} = (7),$$

&c.

Then (3), (4), (5) are respectively prime functions of  $\beta$ ,  $\gamma$ ,  $\delta$ ; that is they cannot be decomposed into factors, rational functions of these quantities; and it is convenient to denote this by writing (3)=[3], (4)=[4], (5)=[5]. But by what precedes, (6) contains the factor (3), that is [3]; and if the other factor, which is prime, is denoted by [6], then we have (6)=[6] [3]. The next term (7) is prime, that is we have (7)=[7]; but the term (8) gives (8)=[8] [4]; the term (9) gives (9)=[9] [3], and so on. Thus we have (12)=[12] [6] [4] [3], the numbers in [ ] being all the factors, the number itself included, and as well composite as prime, of the number in ( ), the factors 2 and 1 being however excluded. To make this clearer, it may be remarked that the last-mentioned equation has the geometrical signification that the relation for a dodecagon is the aggregate of the relations for a proper dodecagon, a proper hexagon, a quadrangle, and a triangle; that is, the relation for a dodecagon implies one or other of the last-mentioned relations. The relations for the several polygons up to the enneagon are in the memoir obtained in a form which puts in evidence the property in question, that is, the series of equations

$$\begin{aligned}(3) &= [3], \\(4) &= [4], \\(5) &= [5], \\(6) &= [6] [3], \\(7) &= [7], \\(8) &= [8] [4], \\(9) &= [9] [3].\end{aligned}$$

To do this, the discriminant is represented, not as above in terms of the constants  $\beta$ ,  $\gamma$ ,  $\delta$ , but in a somewhat different form, by means of the constants  $b$ ,  $c$ ,  $d$ , the last two whereof are such that  $c=0$  is the relation for the triangle,  $d=0$  the relation for the quadrangle; thus  $[3]=c$ ,  $[4]=d$ , and for the particular cases considered, the analytical theorem consists herein, that  $c$  is a factor of (6), and of (9), and that  $d$  is a factor of (8). I have, for the sake of homogeneity, introduced into the formulæ the quantity  $a(=1)$ , but this is a matter of form only.

The functions [3], [4], &c. have been spoken of as *prime*; they are so, in fact, as far they are calculated; and that they are so in general rests on the assumption that for a polygon of a given number of



sides, there is but one form of relation : if, for instance, in the equation  $[12]=0$ , which is the condition for a proper dodecagon, the function  $[12]$  could be decomposed into rational factors ; then equating each of these factors to zero, we should have so many distinct forms of relation for a proper dodecagon. I believe that the assumption and reasoning are valid ; but without entering further into this, I take it for granted that in the general case the functions  $[3]$ ,  $[4]$ , &c. are in fact prime. But the coefficients  $\beta$ ,  $\gamma$ ,  $\delta$ , or  $b$ ,  $c$ ,  $d$ , instead of being so many independent arbitrary quantities, may be given as rational functions of other quantities (if, for instance, the two conics are circles, radii  $R$ ,  $r$ , and distance between the centres  $a$ , then  $\beta$ ,  $\gamma$ ,  $\delta$  will be functions of  $R$ ,  $r$ ,  $a$ ) : and it is in a case of this kind quite conceivable that the functions  $[3]$ ,  $[4]$ , &c., considered as functions of these new elements, should cease to be prime functions. In fact, in the case just referred to of the two circles (the original case of the Porism as considered by Füss), the functions  $[4]$ ,  $[6]$ , &c., which correspond to a polygon of an *even* number of sides, appear to be each of them decomposable into two factors : the memoir contains some remarks tending to show *a priori* that in the case in question this decomposition takes place. I was led to examine the point by the elegant formulæ obtained in an essentially different manner by M. Mention, Bull. de l'Acad. de St. Pétersbourg, t. i. pp. 15, 30 and 507 (1860), in reference to the case of the two circles (it thereby appears that the decomposition takes place for the quadrangle and the hexagon) ; and these formulæ are reproduced in the memoir.

II. "On a New Auxiliary Equation in the Theory of Equations of the Fifth Order." By ARTHUR CAYLEY, Esq., F.R.S.  
Received February 20, 1861.

(Abstract.)

Considering the equation of the fifth order, or quintic equation,  $(x^5 - 1) = (x - x_1)(x - x_2)(x - x_3)(x - x_4)(x - x_5) = 0$ , and putting as usual  $f\omega = x_1 + \omega x_2 + \omega^2 x_3 + \omega^3 x_4 + \omega^4 x_5$ , where  $\omega$  is an imaginary fifth root of unity, then, according to Lagrange's general theory for the solution of equations,  $f\omega$  is the root of an equation of the order 24, called the Resolvent Equation, but the solution whereof depends

ultimately on an equation of the sixth order, viz.  $(f\omega)^5, (f\omega^2)^5, (f\omega^3)^5, (f\omega^4)^5$  are the roots of an equation of the fourth order, each coefficient whereof is determined by an equation of the sixth order; and moreover the other coefficients can be all of them rationally expressed in terms of any one coefficient assumed to be known; the solution thus depends on a single equation of the sixth order. In particular the last coefficient, or  $(f\omega \cdot f\omega^2 \cdot f\omega^3 \cdot f\omega^4)^5$ , is determined by an equation of the sixth order; and not only so, but its fifth root, or  $f\omega \cdot f\omega^2 \cdot f\omega^3 \cdot f\omega^4$  (which is a rational function of the roots, and is the function called by Mr. Cockle the Resolvent Product), is also determined by an equation of the sixth order: this equation may be called the Resolvent-Product Equation. But the recent researches of Mr. Cockle and Mr. Harley\* show that the solution of an equation of the fifth order may be made to depend on an equation of the sixth order, originating indeed in, and closely connected with, the resolvent-product equation, but of a far more simple form: this is the auxiliary equation referred to in the title of the present memoir. The connexion of the two equations, and the considerations which led to the new one, are pointed out in the memoir; but I will here state synthetically the construction of the auxiliary equation. Representing for shortness the roots  $x_1, x_2, x_3, x_4, x_5$ , of the given quintic equation by 1, 2, 3, 4, 5, and putting moreover

$$12345 = 12 + 23 + 34 + 45 + 51, \text{ \&c.}$$

(where on the right-hand side 12, 23, &c. stand for  $x_1x_2, x_2x_3$ , &c.), then the auxiliary equation, say

$$(*\chi\phi, 1)^6 = 0$$

has for its roots

$$\phi_1 = 12345 - 24135, \quad \phi_4 = 21435 - 13245,$$

$$\phi_2 = 13425 - 32145, \quad \phi_5 = 31245 - 14325,$$

$$\phi_3 = 14235 - 43125, \quad \phi_6 = 41325 - 12435,$$

and, it follows therefrom, is of the form

$$(1, 0, C, 0, E, F, G\chi\phi, 1)_6 = 0,$$

\* Cockle, "Researches in the Higher Algebra," Manchester Memoirs, t. xv. pp. 131-142 (1858).

Harley, "On the Method of Symmetric Products, and its Application to the Finite Algebraic Solution of Equations," Manchester Memoirs, t. xv. pp. 172-219 (1859).

Harley, "On the Theory of Quintics," Quart. Math. Journal, t. iii. pp. 343-359 (1859).

where C, E, G are rational and integral functions of the coefficients of the given equation, being in fact seminvariants, and F is a mere numerical multiple of the square root of the discriminant.

The roots of the given quintic equation are each of them rational functions of the roots of the auxiliary equation, so that the theory of the solution of an equation of the fifth order appears to be now carried to its extreme limit. We have in fact

$$\phi_1\phi_5 + \phi_2\phi_4 + \phi_3\phi_6 = (*\chi_{x_1}, 1)^4,$$

$$\phi_1\phi_2 + \phi_3\phi_4 + \phi_5\phi_6 = (*\chi_{x_2}, 1)^4,$$

$$\phi_1\phi_3 + \phi_2\phi_5 + \phi_4\phi_6 = (*\chi_{x_3}, 1)^4,$$

$$\phi_1\phi_4 + \phi_2\phi_6 + \phi_3\phi_5 = (*\chi_{x_4}, 1)^4,$$

$$\phi_1\phi_6 + \phi_2\phi_3 + \phi_4\phi_5 = (*\chi_{x_5}, 1)^4,$$

where  $(*\chi_{x_i}, 1)^4$ , &c. are the values, corresponding to the roots  $x_1$ , &c. of the given equation, of a given quartic function. And combining these equations respectively with the quintic equations satisfied by the roots  $x_1$ , &c. respectively, it follows that, conversely, the roots  $x_1$ ,  $x_2$ , &c. are rational functions of the combinations  $\phi_1\phi_5 + \phi_2\phi_4 + \phi_3\phi_6$ ,  $\phi_1\phi_2 + \phi_3\phi_4 + \phi_5\phi_6$ , &c. respectively, of the roots of the auxiliary equation.

It is proper to notice that, combining together in every possible manner the 6 roots of the auxiliary equation, there are in all 15 combinations of the form  $\phi_1\phi_2 + \phi_3\phi_4 + \phi_5\phi_6$ . But the combinations occurring in the above-mentioned equations are a completely determinate set of five combinations: the equation of the order 15, whereon depend the combinations  $\phi_1\phi_2 + \phi_3\phi_4 + \phi_5\phi_6$ , is not rationally decomposable into three quintic equations, but only into a quintic equation having for its roots the above-mentioned five combinations, and into an equation of the tenth order, having for its roots the other ten combinations, and being an irreducible equation. Suppose that the auxiliary equation and its roots are known; the direct method of ascertaining what combinations of roots correspond to the roots of the quintic equation would be to find the rational quintic factor of the equation of the fifth order, and observe what combinations of the roots of the auxiliary equation are also roots of this quintic factor. The direct calculation of the auxiliary equation by the method of symmetric functions would, I imagine, be very laborious. But the

coefficients are seminvariants, and the process explained in my memoir on the Equation of Differences was therefore applicable, and by means of it, the equation is readily obtained. The auxiliary equation gives rise to a corresponding covariant equation, which is given at the conclusion of the memoir.

### III. "On Combustion in Rarefied Air." By Dr. EDWARD FRANKLAND, F.R.S. Received February 28, 1861.

In the autumn of 1859, whilst accompanying Dr. Tyndall to the summit of Mont Blanc, I undertook at his request some experiments on the effect of atmospheric pressure upon the amount of combustible matter consumed by a common candle. I found that, taking the average of five experiments, a stearin candle diminished in weight 9·4 grammes when burnt for an hour at Chamounix; whilst its ignition for the same length of time on the summit of Mont Blanc, perfectly protected from currents of air, reduced its weight to the extent of 9·2 grammes.

This close approximation to the former number under such a widely different atmospheric pressure, goes far to prove that the rate of combustion is entirely independent of the density of the atmosphere.

It is impossible to repeat these determinations in a satisfactory manner with artificially rarefied atmospheres, owing to the heating of the apparatus which surrounds the candle, and the consequent guttering and unequal combustion of the latter; but an experiment in which a sperm candle was burnt first in air under a pressure of 28·7 inches of mercury, and then in air at 9 inches pressure, other conditions being as similar as possible in the two experiments, the consumption of sperm was found to be,—

At pressure of 28·7 inches 7·85 grms. of sperm per hour,

„ 9·0 „ 9·10 „ „

thus confirming, for higher degrees of rarefaction, the result previously obtained.

In burning the candles upon the summit of Mont Blanc, I was much struck by the comparatively small amount of light which they emitted. The lower and blue portion of the flame, which under

ordinary circumstances scarcely rises to within a quarter of an inch of the apex of the wick, now extended to the height of  $\frac{1}{8}$ th of an inch above the cotton, thus greatly reducing the size of the luminous portion of the flame.

On returning to England, I repeated the experiments under circumstances which enabled me to ascertain, by photometrical measurements, the extent of this loss of illuminating effect in rarefied air. The results prove that a great reduction in the illuminating power of a candle ensues when the candle is transferred from air at the ordinary atmospheric pressure to rarefied air. It was, however, found that, owing to the circumstances mentioned above, no satisfactory quantitative experiments could be made with candles in artificially rarefied air, and recourse was therefore had to coal-gas, which, although also liable to certain disturbing influences, yet yielded results, during an extensive series of experiments, exhibiting sufficient uniformity to render them worthy of confidence. The gas was in all cases passed through a governor to secure uniformity of pressure in the delivery tubes. A single jet of gas was employed as the standard of comparison, and this was fixed at one end of a Bunsen's photometer, whilst the flame to be submitted to various pressures, and which I will call the experimental flame, was placed at the other. The experimental flame was made to burn a uniform amount of gas, viz. 0.65 cubic foot per hour in all the experiments.

The products of combustion were completely removed, so that the experimental flame, which burnt with perfect steadiness, was always surrounded with pure air, the supply of which was, however, so regulated as to secure a maximum of illuminating effect in each observation.

In all the following series of experiments, the illuminating power given under each pressure is the average of twenty observations, which accord with each other very closely. In each series, the maximum illuminating effect, that is the light given by the experimental flame when burning under the full atmospheric pressure, is assumed to be 100. The following is a summary of the results:—

## 1st Series.

Pressure of air in inches of mercury.	Illuminating power of experimental flame.
29.9	100.
24.9	75.0
19.9	52.9
14.6	20.2
9.6	5.4
6.6	.9

## 2nd Series.

30.2	100.
28.2	91.4
26.2	80.6
24.2	73.0
22.2	61.4
20.2	47.8
18.2	37.4
16.2	29.4
14.2	19.8
12.2	12.5
10.2	3.6

These numbers indicate that even the natural oscillations of atmospheric pressure must produce a considerable variation in the amount of light emitted by gas-flames, and it was therefore important to determine, by a special series of observations, this variation in luminosity within, or nearly within, the usual fluctuations of the barometrical column. In order to attain greater delicacy in the pressure readings in these experiments, a water-gauge was used, but its indications are translated into inches of mercury in the following tabulated results, each of which represents, as before, the average of twenty observations.

## 3rd Series.

Pr. of air in in. of mercury.	Illum. power of exp. flame.
30.2	100.
29.2	95.0
28.2	89.7
27.2	84.4

It is thus evident that the combustion of an amount of gas which would give a light equal to 100 candles when the barometer stands at 31 inches, would give a light equal to only 84.4 candles if the barometer fell to 28 inches.

An inspection of all the above results shows that the rarefaction of air, from atmospheric pressure downwards, produces a uniformly diminishing illuminating power until the pressure is reduced to about 14 inches of mercury, below which the diminution of light proceeds at a less rapid rate. The above determinations give approximately 5·1 per cent. as the mean reduction of light for each diminution of 1 inch of mercurial pressure down to 14 inches. The following Table exhibits the actually observed light, compared with that calculated from this constant.

## 1st Series.

Pressure.	Illuminating power.	
	Observed.	Calculated.
29·9	100·	100·
24·9	75·0	74·5
19·9	52·9	49·0
14·6	20·2	22·0
9·6	5·4	— 3·5
6·6	·9	— 18·8

## 2nd Series.

30·2	100·	100·
28·2	91·4	89·8
26·2	80·6	79·6
24·2	73·0	69·4
22·2	61·4	59·2
20·2	47·8	49·0
18·2	37·4	38·8
16·2	29·4	28·6
14·2	19·8	18·4
12·2	12·5	8·2
10·2	3·6	— 2·0

## 3rd Series.

30·2	100·	100·
29·2	95·0	94·9
28·2	89·7	89·8
27·2	84·4	84·7

I am now extending this inquiry to pressures exceeding that of the atmosphere, and hope soon to lay before the Society the detailed results of the whole series, together with some observations on the causes of this variation of luminosity.

March 14, 1861.

Major-General SABINE, R.A., Vice-President and Treasurer,  
in the Chair.

The following communications were read :—

- I. "On an Application of the Theory of Scalar and Clinant Radical Loci." By ALEXANDER J. ELLIS, Esq., B.A., F.C.P.S. Communicated by ARTHUR CAYLEY, Esq. Received February 20, 1861.

(Abstract.)

This investigation is in correction and extension of Plücker's theory of transversals (*System der Geometrie*, § 3, art. 64), and is founded on the theories explained in the 'Proceedings,' vol. x. pp. 415-426.

It is shown that if  $f(x, y)$  be an algebraical formation (function) of  $n + 2m$  dimensions, such that when  $x$  is scalar (possible)  $f=0$  has  $n$  scalar and  $2m$  clinant (imaginary) roots, and  $\lambda$  is the coefficient of  $y^{n+2m}$ , the value of  $f(x, y_1)$  may be represented geometrically by

$$\lambda \cdot \frac{M_1 Q}{OB} \cdot \frac{M_2 Q}{OB} \dots \frac{M_n Q}{OB} \times \frac{M'_1 Q}{OB} \cdot \frac{M'_2 Q}{OB} \dots \frac{M'_1^{(m)} Q}{OB} \cdot \frac{M'_2^{(m)} Q}{OB},$$

where O is the origin,  $OP=x_1 \cdot OI$ ,  $PQ=y_1 \cdot OB$ , and PQ is any straight line, cutting the curve whose equations are

$$OM=x \cdot OI + y \cdot OB, \quad f(x, y)=0,$$

(where  $x, y$  are scalar, OI is in the direction OP, OB is of the same length as OI and in the direction PQ) in the  $n$  points  $M_1, M_2 \dots M_n$ , and where  $M'_1, M'_2 \dots M'_1^{(m)}, M'_2^{(m)}$  are determined as follows.

Put  $y=r + \sqrt{-1} \cdot s$ , where  $r$  and  $s$  are scalar, reduce  $f(x, y)$  to the form  $F_1(x, r, s) + \sqrt{-1} \cdot s \cdot F_2(x, r, s)$ , put  $F_1=0, F_2=0$ , from which equations find by elimination  $F_3(x, r)=0, F_4(x, s)=0$ , and construct the loci of R and S, where

$$\begin{aligned} OR &= x \cdot OI + r \cdot OB, & F_3(x, r) &= 0, \\ OS &= x \cdot OI + s \cdot OB, & F_4(x, s) &= 0. \end{aligned}$$



Then determine the points  $R', R'' \dots R^{(m)}$  in which PQ cuts the first, and the corresponding pairs of points  $S_1', S_2', S_1'', S_2'' \dots S_1^{(m)}, S_2^{(m)}$  in which it cuts the second, and draw  $R'M_1' = \sqrt{-1} \cdot PS_1',$   
 $R'M_2' = \sqrt{-1} \cdot PS_2', \dots \dots R^{(m)} M_1^{(m)} = \sqrt{-1} \cdot PS_1^{(m)},$   
 $R^{(m)} M_2^{(m)} = \sqrt{-1} \cdot PS_2^{(m)}.$

The loci of M, R, S are the principal scalar and clinant radical loci of the formation  $f(x, y).$

From this it is concluded, that if  $O_1 O_2 \dots O_n O_1$  be a completely enclosed polygon, the sides of which are taken in order, to replace PQ, first in the direction  $O_1 O_2, O_2 O_3, \&c.,$  and then in the direction  $O_2 O_1, O_3 O_2, \&c.,$  and the points  $M_1, M_2 \dots M_n, M_1', M_2' \dots M_1^{(m)}, M_2^{(m)}$  be determined for each direction and each side, and  $[O_{1,2} M + O_{2,1} M]$  represent the product of the ratios  $O_1 M + O_2 M,$  &c., the  $O_1 M$  referring to the origin  $O_1$  and the  $M$  being determined with reference to the direction  $O_1 O_2,$  and the  $O_2 M$  referring to the origin  $O_2$  and the  $M$  being determined with reference to the direction  $O_2 O_1,$  and so on, then we shall have in all cases,

$$[O_{1,2} M + O_{2,1} M] \cdot [O_{2,3} M + O_{3,2} M] \dots [O_{n,1} M + O_{1,n} M] = 1,$$

and not  $= \pm 1,$  as supposed by Plücker, whose error is traced to its origin, and displayed in all the examples he has given, where the roots of  $f=0$  are scalar. This result is then applied to a simple case where some of the roots of  $f=0$  are clinant, and a result obtained in accordance with other considerations.

## II. "A Seventh Memoir on Quantics." By ARTHUR CAYLEY, Esq., F.R.S. Received February 28, 1861.

(Abstract.)

The present memoir relates chiefly to the theory of ternary cubics. Since the date of my Third Memoir on Quantics, M. Aronhold has published the continuation of his researches on ternary cubics, in the memoir "Theorie der homogenen Functionen dritten Grades von drei Veränderlichen," Crelle, t. lv. pp. 97-191 (1858). He there considers two derived contravariants, linear functions of the fundamental ones, and which occupy therein the position which the funda-

mental contravariants PU, QU do in my third memoir ; in the notation of the present memoir these derived contravariants are

$$\begin{aligned} YU &= 3T. \quad PU - 48. QU, \\ ZU &= -48S^2. PU + T. QU ; \end{aligned}$$

and for the canonical form  $x^3 + y^3 + z^3 + 6lxyz$ , they acquire respectively the factor  $(1 + 8l^2)^2$ , viz. in this case

$$\begin{aligned} YU &= (1 + 8l^2)^2 \{ l \quad (\xi^2 + \eta^2 + \zeta^2) - 3\xi\eta\zeta \} \\ ZU &= (1 + 8l^2)^2 \{ (1 + 2l^2) (\xi^2 + \eta^2 + \zeta^2) + 18l^2\xi\eta\zeta \}. \end{aligned}$$

The derived contravariants have with the covariants U, HU, even a more intimate connexion than have the contravariants PU, QU ; and the advantage of the employment of YU, ZU fully appears by M. Aronhold's memoir.

But the conclusion is, not that the contravariants PU, QU are to be rejected, but that the system is to be completed by the addition thereto of two derived covariants, linear functions of U, HU ; these derived covariants, suggested to me by M. Aronhold's memoir, are in the present memoir called CU, DU ; their values are

$$\begin{aligned} CU &= -T. U + 24S. HU \\ DU &= 8S^2. U - 3T. HU ; \end{aligned}$$

and for the canonical form  $x^3 + y^3 + z^3 + 6lxyz$ , they acquire respectively, not indeed  $(1 + 8l^2)^2$ , but the simple power  $(1 + 8l^2)$ , as a factor, viz. in this case

$$\begin{aligned} CU &= (1 + 8l^2) \{ (-1 + 4l^2) (x^3 + y^3 + z^3) + 18lxyz \} \\ DU &= (1 + 8l^2) \{ l^2(5 + 4l^2) (x^3 + y^3 + z^3) + 3(1 - 10l^2)xyz \} ; \end{aligned}$$

it was in fact by means of this condition as to the factor  $(1 + 8l^2)$ , that the foregoing expressions for CU, DU were obtained.

The formulæ of my third memoir and those of M. Aronhold are by this means brought into harmony and made parts of a whole ; instead of the two intermediates  $\alpha U + 6\beta HU$ ,  $6\alpha PU + \beta QU$  in Tables 68 and 69 of my Third Memoir, or of the intermediates  $\alpha U + 6\beta HU - 2\alpha YU + 2\beta ZU$  of M. Aronhold's theory, we have the four intermediates  $\alpha U + 6\beta HU$ ,  $-2\alpha YU + 2\beta ZU$ ,  $2\alpha CU - 2\beta DU$ ,  $6\alpha PU + \beta QU$  in Tables 74, 75, 76, and 77 of the present memoir. These four tables embrace the former results, and the new ones which relate to the covariants CU, DU ; and they are what is most important in the pre-

sent memoir. I have, however, excluded from the Tables, and I do not in the memoir consider (otherwise than incidentally) the covariant of the sixth order  $\Theta U$ , or the contravariant (reciprocant)  $FU$ .

I have given in the memoir a comparison of my notation with that of M. Aronhold. A short part of the memoir relates to the binary cubic and the binary quartic, viz. each of these quantics has a covariant of its own order, forming with it an intermediate  $\alpha U + \beta W$ , the covariants whereof contain quantics in  $(\alpha, \beta)$ , the coefficients of which are invariants of the original quantic. The formulæ which relate to these cases are in fact given in my Fifth Memoir, but they are reproduced here in order to show the relations between the quantics in  $(\alpha, \beta)$  contained in the formulæ. As regards the binary quartic, these results are required for the discussion of the like question in regard to the ternary cubic, viz. that of finding the relations between the different quantics in  $(\alpha, \beta)$  contained in the formulæ relating to the ternary cubic. Some of these relations have been obtained by M. Hermite in the memoir "Sur les formes cubiques à trois indéterminées," Liouville, t. iii. pp. 37-40 (1858), and in that "Sur la Résolution des équation du quatrième degré," Comptes Rendus, t. xlv. p. 715 (1858), and by M. Aronhold in his memoir already referred to; and in particular I reproduce and demonstrate some of the results in the last mentioned memoir of M. Hermite. But the relations in question are in the present memoir exhibited in a more complete and systematic form.

### III. "On the Secular Change in the Magnetic Dip in London, between the years 1821 and 1860." By Major-General EDWARD SABINE, R.A., Treas. and V.P.R.S. Received March 7, 1861.

I propose in this communication to bring together and discuss four determinations at different epochs, in which I have myself been either directly or indirectly concerned, which have had expressly in view the object which forms the title of the paper.

*Epoch of 1821.*—The experiments on this occasion were made in a part of the Regent's Park, then occupied as the nursery garden of Mr. Jenkins: an unexceptionable locality in all respects, and far

distant at that time from buildings or iron implements, railing, or pipes. The experiments, ten in number, were made on six different days, between the 3rd and 10th of August 1821; and all between 8 A.M. and 4 P.M. The circle employed was  $11\frac{1}{2}$  inches in diameter, made by Nairne, a celebrated artist in his day for instruments of this description: the needle was made by Dollond on Professor Tobias Meyer's principle, described in the Göttingen Transactions for 1814. The size of the small spheres, or their distance from the needle, was varied in the different experiments, so as to bring different parts of the axle to rest on the agate planes. The mean of the ten experiments was  $70^{\circ} 02' 9''$  N., corresponding to the epoch 1821.65: the extremes being  $70^{\circ} 00' 1''$  and  $70^{\circ} 05' 9''$ . The whole of the experiments were made by myself, and are detailed in a paper in the Phil. Trans. for 1822, Art. 1.

*Epoch of 1838.*—The experiments on this occasion were made on different days in 1837 and 1838, in the course of the magnetic survey of Great Britain, by Messrs. Robert Were Fox and John Phillips, Captain (since Admiral) Sir James Clark Ross, Captain Edward Johnson of the Royal Navy, and myself. The instruments employed were those of Robinson, Gambey, and Jordan: the particulars are recorded in the 8th volume of the Reports of the British Association for the Advancement of Science (1839), Table 10, p. 64. The localities in which the experiments were made were—1. The same spot in the Regent's Park where those of 1821 had been made. 2. Kew Gardens. 3. Westbourne Green, a locality which has been since built over. Separate determinations were made on 13 days between May 30, 1837, and December 10, 1838, the mean epoch being 1838.3, and the mean dip  $69^{\circ} 17' 3''$  N. The extremes of all the observers and of all the instruments were  $69^{\circ} 13' 3''$ , and  $69^{\circ} 23' 9''$ .

*Epoch of 1854.*—The experiments on which this determination rests were made by the late Mr. John Welsh, of the Kew Observatory, and myself in August and September 1854, with two inclinometers made by Mr. Henry Barrow (successor to Mr. Robinson), fitted according to the modern English construction with verniers and microscopes, and each having two needles. The localities selected were—1. The station in the Regent's Park already named as that of the experiments in 1821, and of a part of those in 1838; and 2. the magnetic house of the Kew Observatory. The experiments had a

double purpose, viz. 1, to ascertain the difference, if any, in the dip in the Regent's Park and in the magnetic house at Kew; and 2, to obtain a determination of the dip in August 1854 which might be strictly comparable with the result obtained in August 1821. The experiments were made on five different days, and comprised eighteen determinations, ranging between  $68^{\circ} 29' 25$  and  $68^{\circ} 33' 73$ ; the mean being  $68^{\circ} 31' 13$  N., corresponding to the epoch 1854.65. The mean of eight determinations in the Regent's Park was  $68^{\circ} 30' 55$ , and of ten determinations at Kew,  $68^{\circ} 31' 6$ ; the difference of either from the mean being  $0' 52$ , which is within the limits of probable error. A detailed notice of these experiments was published in 1855 in an Editor's note in p. 364 of the translation, edited by myself, of Arago's *Meteorological Essays*.

*Epoch of 1859.5.*—The dip corresponding to July 1, 1859 (now first discussed), is derived from 282 determinations made in the magnetic house at Kew on 121 different days between November 1857 and December 1860 inclusive, chiefly by four observers, viz. Mr. John Welsh, late Director of the Kew Observatory, Mr. Balfour Stewart, its present Director, Mr. Chambers, Assistant in the Observatory, and Dr. Bergsma, Director of the Magnetical and Meteorological Observatory of the Netherlands Government in Java.

There were employed in these determinations, on different occasions, twelve circles and twenty-four needles, all of the same form and pattern; the circles being 6 inches in diameter, fitted with verniers and microscopes, and the needles  $3\frac{1}{2}$  inches in length; they were all made by Mr. Henry Barrow. Every determination was complete in the eight different positions of the circle and needle, as described in Appendix 2 of the Article "Terrestrial Magnetism" in the 3rd edition of the 'Admiralty Manual of Scientific Inquiry.' The individual results are shown in the subjoined Tables, whereof Table I. contains 115 determinations comprised between November 1857 and December 1858; Table II. 96 determinations between January and December 1859; and Table III. 71 determinations between January and December 1860. The results in each year are reduced to the 1st of July in the same year, employing the proportional parts of an annual secular change of  $-2' 6$ : those which were obtained in the four winter months, November, December, January, and February, have also received a correction of  $-0' 8$ , and those obtained in the summer

months, May, June, July, and August, a correction of  $+0\cdot8$  in compensation for annual variation, agreeably to an investigation contained in the sequel. The Tables exhibit in every case the date, the particular circle and needle employed, the azimuths in which the observations were made, the name of the observer, the observed dip, the reduction to a common epoch, the correction for annual variation, and finally, the corrected result.

The opportunity afforded at the Kew Observatory, of testing the degree of accordance which may be expected in the results of different instruments constructed on the plan which has been for several years past approved and adopted at Kew, has thus been profited by, and the conclusions appear such as to merit the consideration of those who are desirous to possess reliable instruments. Several of the circles are the property of foreign governments or of individuals, at whose request they were provided subject to a verification at Kew. The observations here recorded were for the most part made for the purpose of such verifications, and were entered as they were made in the books of the Kew Observatory, from which they are now taken. No observation has been omitted. The circles were distinguished by the numbers 20, 23, 27, 28, 30, 31, 33, 34, 35 and 36; and two unnumbered, one known as the Kew Circle, the other an inclinometer employed by Admiral Sir James Clark Ross in his recent magnetic survey of a part of England. No. 20 was made for Professor Hansteen of Christiania, and is now in his possession; 23 is the circle used by the late Mr. Welsh in his magnetic survey of Scotland; 27 was supplied to the Austrian frigate 'Novara' for her voyage of circumnavigation; 28 was made for the Russian Government; 30 was used by myself in the recent magnetic survey of England, and has been since supplied to the Observatory at the "Isle Jesus" near Montreal in Canada; 31 was made for Padre Secchi of the Collegio Romano, and is now at Rome; 32 was made for the Rev. Alfred Weld of Stonyhurst College, and is now in the Observatory of that College; 34 was supplied to the Government of the United States of America, and is now in the possession of Dr. Alexander Dallas Bache, Superintendent of the Coast Survey; Nos. 35 and 36 were made for the Netherlands Government, one for Utrecht, and one for Java; the "Kew Circle" was in regular use for the monthly determinations of the Dip at Kew, from the commencement of those observations until

August 1859, when it was exchanged for No. 33, which has subsequently been, and is now, in regular employment for that purpose.

Besides the four principal observers already noticed, a few determinations were made, as is shown in the Tables, by Mr. Valentine Magrath, Assistant in the Observatory, by Captain Haig of the Royal Artillery, practising at Kew preparatory to his employment on the Boundary Commission between the United States and the British possessions on the West Coast of North America, and by Lieut. Goodall of the Royal Engineers, who attended at Kew to practise the manipulation of magnetical instruments.

TABLE I.—Observations of the Magnetic Dip, at the Kew Observatory, in 1857 and 1858, with Circles of the English Construction, fitted with Verniers and Microscopes.

Date.	Circle.	Needle.	Azimuth.	Observer.	Observed Dip.	Reduction to Epoch.	Correction for Annual Variation.	Corrected Dip, July 1, 1859.
1857. Nov.								
2	27	1	0° & 180°	Mr. Welsh.	68° 23'4	-1'8	-0'8	68° 20'8
2	27	1	30 " 120	"	24'9	1'8	0'8	22'3
2	27	1	60 " 150	"	24'5	1'8	0'8	21'9
2	27	2	0 " 180	"	25'2	1'8	0'8	22'6
2	27	2	30 " 120	"	23'8	1'8	0'8	21'2
2	27	2	60 " 150	"	24'0	1'8	0'8	21'4
2	Kew	2	0 " 180	Mr. Chambers.	27'0	1'8	0'8	24'4
2	Kew	2	30 " 120	"	24'0	1'8	0'8	21'4
2	Kew	2	60 " 150	"	24'0	1'8	0'8	21'6
3	23	1	0 " 180	Mr. Welsh.	26'3	1'8	0'8	23'7
3	23	1	30 " 120	"	26'6	1'8	0'8	24'0
3	23	1	60 " 150	"	25'6	1'8	0'8	23'0
3	23	2	0 " 180	"	26'6	1'8	0'8	24'0
3	23	2	30 " 120	"	25'5	1'8	0'8	22'9
3	23	2	60 " 150	"	25'0	1'8	0'8	22'4
3	27	3	0 " 180	Mr. Chambers.	22'0	1'8	0'8	19'4
3	27	3	30 " 120	"	23'5	1'8	0'8	20'9
3	27	3	60 " 150	"	23'5	1'8	0'8	20'9
4	28	1	0 " 180	"	22'3	1'8	0'8	19'7
4	28	1	30 " 120	"	25'1	1'8	0'8	22'5
4	28	1	60 " 150	"	27'2	1'8	0'8	24'6
5	28	2	0 " 180	"	28'1	1'8	0'8	25'5
5	28	2	30 " 120	"	25'3	1'8	0'8	22'7
5	28	2	60 " 150	"	28'1	1'8	0'8	25'5
7	28	1 & 4	Mer., and at right angles.	"	25'6	1'8	0'8	23'0
10	27	1 & 4	Mer., and at right angles.	"	34'2	1'8	0'8	31'6
27	28	1	0° & 180°	"	24'9	1'4	0'8	22'7
27	28	2	"	"	68 29'4	-1'4	-0'8	68 27'2

TABLE I. (continued.)

Date.	Circle.	Needle.	Azimuth.	Observer.	Observed Dip.	Reduction to Epoch.	Correction for Annual Variation.	Corrected Dip, July 1, 1888.
1858.								
Jau. 1	Kew	1	0° & 180°	Mr. Chambers.	68° 20'7"	-1'4"	-0'8"	68° 18'5"
1	Kew	2	"	"	24'7"	1'4"	0'8"	22'5"
5	Kew	2	"	"	24'6"	1'4"	0'8"	22'4"
5	Kew	1	"	Mr. Welsh.	20'8"	1'4"	0'8"	18'6"
8	Kew	1	"	Cap. Bedingfield, R.N.	25'1"	1'4"	0'8"	22'9"
14	Kew	2	"	Capt. Haig, R.A.	24'9"	1'3"	0'8"	22'8"
14	Kew	2	"	"	24'6"	1'3"	0'8"	22'5"
16	Kew	1	"	"	23'6"	1'3"	0'8"	21'5"
18	23	1	"	"	28'4"	1'3"	0'8"	26'3"
18	23	1	"	"	22'8"	1'3"	0'8"	20'7"
Feb. 3	?	1	"	Mr. Chambers.*	27'2"	1'2"	0'8"	25'2"
4	Kew	1	"	"	22'9"	1'2"	0'8"	20'9"
4	Kew	2	"	"	26'9"	1'2"	0'8"	24'9"
27	Kew	1	"	"	20'3"	1'0"	0'8"	18'5"
27	Kew	2	"	"	22'5"	1'0"	-0'8"	20'7"
Mar. 1	Kew	1	"	Mr. Welsh.	24'4"	1'0"	0'0"	23'4"
1	Kew	2	"	"	24'3"	1'0"	0'0"	23'3"
4	Kew	2	"	"	25'4"	0'9"	0'0"	24'5"
5	Kew	1	"	"	22'5"	0'9"	0'0"	21'6"
22	Kew	1	"	"	23'7"	0'7"	0'0"	23'0"
22	Kew	2	"	"	25'9"	0'7"	0'0"	25'2"
27	Kew	1	"	"	23'7"	0'7"	0'0"	23'0"
27	Kew	2	"	"	26'1"	0'7"	0'0"	25'4"
27	Kew	1	"	Mr. Chambers.	21'6"	0'7"	0'0"	20'9"
30	20	1	"	Mr. Welsh.	25'4"	0'7"	0'0"	24'7"
30	30	1	30° & 120°	"	28'3"	0'7"	0'0"	27'6"
30	30	1	60° " 150°	"	23'0"	0'7"	0'0"	22'3"
30	30	1	0° " 180°	"	23'2"	0'7"	0'0"	22'5"
30	20	1	30° " 120°	"	21'3"	0'7"	0'0"	20'6"
30	30	1	60° " 150°	"	25'2"	0'7"	0'0"	24'5"
30	30	2	0° " 180°	Mr. Chambers.	25'1"	0'7"	0'0"	24'4"
30	30	2	30° " 120°	"	26'9"	0'7"	0'0"	26'2"
30	30	2	60° " 150°	"	27'0"	0'7"	0'0"	26'3"
Apr. 22	Kew	2	0° " 180°	Mr. Magrath.	23'7"	0'5"	0'0"	23'2"
22	Kew	1	"	"	24'0"	0'5"	0'0"	23'5"
27	Kew	1	"	Mr. Welsh.	20'9"	0'5"	0'0"	20'4"
27	Kew	2	"	"	21'7"	0'5"	0'0"	21'2"
28	Kew	1	"	Mr. Chambers.	22'8"	0'5"	0'0"	22'3"
28	Kew	2	"	"	21'7"	0'5"	0'0"	21'2"
May 20	Kew	1	"	"	22'3"	0'3"	+0'8"	22'8"
20	Kew	2	"	"	23'9"	0'3"	0'8"	24'4"
26	Kew	1	"	Mr. Welsh.	20'5"	0'3"	0'8"	21'0"
26	Kew	2	"	"	24'1"	0'3"	0'8"	24'6"
27	Kew	1	"	Mr. Chambers.	21'6"	0'3"	0'8"	22'1"
27	Kew	2	"	"	21'3"	0'3"	0'8"	21'8"
27	Sir J. Ross	1	"	Mr. Welsh.	25'4"	0'3"	0'8"	25'9"
28	Sir J. Ross	1	"	"	26'8"	0'3"	0'8"	27'3"
28	Sir J. Ross	1	30° & 120°	"	20'3"	0'3"	0'8"	20'8"
28	Sir J. Ross	1	60° " 150°	"	24'0"	0'3"	0'8"	24'5"
June 9	30	1	0° " 180°	"	68° 21'1"	-0'2"	+0'8"	68° 21'7"

\* Marked "Doubtful."



TABLE I. (continued.)

Date.	Circle.	Needle.	Azimuth.	Observer.	Observed Dip.	Reduction to Epoch.	Correction for Annual Variation.	Corrected Dip, July 1, 1858.
1858.								
June 9	30	2	0° & 180°	Mr. Welsh.	68° 21'6"	-0'2"	+0'8"	68° 22'2"
22	Kew	1	"	Mr. Chambers.	19'9"	0'1"	0'8"	20'6"
22	Kew	2	"	"	20'7"	0'1"	0'8"	21'4"
22	Kew	2	"	"	19'8"	0'1"	0'8"	20'5"
22	Kew	1	"	"	18'2"	0'1"	0'8"	18'9"
22	23	1	"	Mr. Welsh.	27'4"	0'1"	0'8"	28'1"
22	23	2	"	"	23'0"	0'1"	0'8"	23'7"
22	23	2	"	"	23'1"	0'1"	0'8"	23'8"
22	23	2	30° & 120°	"	25'2"	0'1"	0'8"	25'9"
22	23	2	60° " 150°	"	24'7"	-0'1"	0'8"	25'4"
28	23	2	0° " 180°	"	25'1"	0'0"	0'8"	25'9"
28	Kew	1	"	Mr. Chambers.	23'4"	0'0"	0'8"	24'2"
29	23	2	"	Mr. Welsh.	25'1"	0'0"	0'8"	25'9"
29	23	1	"	"	23'9"	0'0"	0'8"	24'7"
29	23	1	"	Mr. Chambers.	21'2"	0'0"	0'8"	22'0"
July 3	32	1	"	"	24'3"	0'0"	0'8"	25'1"
6	32	1	"	"	25'5"	0'0"	0'8"	26'3"
6	32	2	"	"	22'9"	0'0"	0'8"	23'7"
8	32	2	"	"	22'7"	+0'1"	0'8"	23'6"
8	32	1	"	"	23'1"	0'1"	0'8"	24'0"
Aug. 3	Kew	2	"	"	20'1"	0'3"	0'8"	21'2"
10	Kew	1	"	"	20'7"	0'3"	0'8"	21'8"
10	Kew	2	"	"	21'3"	0'3"	0'8"	22'4"
20	Kew	1	"	"	21'7"	0'4"	0'8"	22'9"
20	Kew	2	"	"	23'5"	0'4"	+0'8"	24'7"
Sept. 16	Kew	1	"	"	22'1"	0'6"	0'0"	22'7"
17	Kew	2	"	"	20'8"	0'6"	0'0"	21'4"
Oct. 13	Kew	1	"	"	23'4"	0'7"	0'0"	24'1"
14	Kew	2	"	"	24'2"	0'7"	0'0"	24'9"
Nov. 4	Kew	1	"	"	20'0"	0'9"	-0'8"	20'1"
4	Kew	2	"	"	23'4"	0'9"	0'8"	23'5"
5	23	1	"	"	25'6"	0'9"	0'8"	28'7"
6	23	2	"	"	26'1"	0'9"	0'8"	26'2"
29	23	2	"	"	23'0"	1'0"	0'8"	23'2"
30	23	1	"	"	24'3"	1'0"	0'8"	24'5"
Dec. 10	Kew	2	"	"	20'8"	1'1"	0'8"	21'1"
29	31	1	"	"	22'3"	1'3"	0'8"	22'8"
29	31	2	"	"	68° 20'4"	+1'3"	-0'8"	68° 20'9"
July 1, 1858, mean of 115 observations .....								68° 23'2"

TABLE II.—Observations of the Magnetic Dip at the Kew  
Observatory in 1859.

Date.	Circ.	Needle.	Aim.	Observer.	Observed Dip.	Reduction to Epoch.	Correction for Annual Variation.	Corrected Dip, July 1, 1859.
1859.								
Jan. 4	31	1	0° & 180°	Mr. Chambers.	68° 22'5	-1'3	-0'8	68° 20'4
4	31	2	"	"	21'0	1'3	0'8	18'9
11	Kew	1	"	Mr. V. Magrath.	23'6	1'3	0'8	21'5
11	30	1	"	Mr. Chambers.	23'7	1'3	0'8	21'6
11	30	1	"	"	24'6	1'3	0'8	22'5
11	Kew	1	"	"	21'6	1'3	0'8	19'5
12	30	2	"	"	23'7	1'3	0'8	21'6
12	Kew	1	"	"	22'2	1'3	0'8	20'1
12	30	2	"	"	24'4	1'3	0'8	22'3
24	30	1	"	"	22'7	1'2	0'8	20'7
24	Kew	2	"	"	19'8	1'2	0'8	17'8
24	30	1	"	"	21'6	1'2	0'8	19'6
25	30	2	"	"	24'1	1'2	0'8	22'1
25	Kew	2	"	"	20'4	1'2	0'8	18'4
25	30	1	"	"	22'2	1'2	0'8	20'2
25	Kew	1	"	"	18'9	1'2	-0'8	16'9
Mar. 7	Kew	1	"	"	19'6	0'8	0'0	18'8
7	Kew	2	"	"	20'1	0'8	0'0	19'3
8	30	1	"	"	21'7	0'8	0'0	20'9
8	30	2	"	"	22'2	0'8	0'0	21'4
8	20	1	"	"	20'8	0'8	0'0	20'0
8	20	2	"	"	22'8	0'8	0'0	22'0
10	20	2	"	"	24'6	0'8	0'0	23'8
10	33	2	"	"	20'5	0'8	0'0	19'7
11	20	1	"	"	24'1	0'8	0'0	23'3
11	33	1	"	"	25'0	0'8	0'0	24'2
11	33	1	"	"	22'2	0'8	0'0	21'4
11	33	2	"	"	22'3	0'8	0'0	21'5
11	34	1	"	"	21'0	0'8	0'0	20'2
11	34	2	"	"	21'6	0'8	0'0	20'8
12	34	1	"	"	21'4	0'8	0'0	20'6
12	34	2	"	"	20'4	0'8	0'0	19'6
12	23	1	"	"	25'0	0'8	0'0	24'2
12	23	2	"	"	21'8	0'8	0'0	21'0
14	23	1	"	"	24'6	0'8	0'0	23'8
14	23	2	"	"	22'7	0'8	0'0	21'9
15	33	1	"	"	22'9	0'8	0'0	22'1
15	33	2	"	"	21'7	0'8	0'0	20'9
15	Kew	1	"	"	21'5	0'8	0'0	20'7
15	Kew	2	"	"	26'3	0'8	0'0	25'5
16	33	1	"	"	22'9	0'8	0'0	22'1
16	33	2	"	"	21'8	0'8	0'0	21'0
16	34	1	"	"	21'5	0'8	0'0	20'7
16	34	2	"	"	19'5	0'8	0'0	18'7
17	34	1	"	"	24'8	0'7	0'0	24'1
17	34	2	"	"	18'4	0'7	0'0	17'7
17	30	1	"	"	21'7	0'7	0'0	21'0
17	30	2	"	"	68° 24'4	-0'7	0'0	68° 23'7

TABLE II. (continued.)

Date.	Circle.	Needle.	Azinuth.	Observer.	Observed Dip.	Reduction to Epoch.	Correction for Annual Variation.	Corrected Dip, July 1, 1859.
1859.								
Mar. 18	20	1	0° & 180°	Mr. Chambers.	68° 23'3	-0'7	0'0	68° 22'6
18	20	2	"	"	25'0	0'7	0'0	24'3
18	Kew	2	"	"	21'5	0'7	0'0	20'8
19	Kew	1	"	"	21'5	0'7	0'0	20'8
19	20	1	"	"	23'7	0'7	0'0	23'0
19	20	2	"	"	26'7	0'7	0'0	26'0
April 20	23	2	"	Dr. Bergama.	22'5	0'5	0'0	22'0
?	?	?	"	"	21'2	0'5	0'0	20'7
?	?	?	"	"	19'2	0'5	0'0	18'7
30	33	2	"	"	21'7	0'5	0'0	21'2
May 16	33	1	"	"	18'8	0'3	+0'8	19'3
16	33	2	"	"	18'1	0'3	0'8	18'6
16	Kew	1	"	Mr. Chambers.	19'0	0'3	0'8	19'5
16	Kew	2	"	"	19'4	0'3	0'8	19'9
17	Kew	1	"	Dr. Bergama.	18'2	0'3	0'8	18'7
17	Kew	2	"	"	20'3	0'3	0'8	20'8
17, 18	33	1	"	Mr. Chambers.	20'7	0'3	0'8	21'2
17, 18	33	2	"	"	20'7	-0'3	0'8	21'2
July 7	23	1	"	"	21'3	0'0	0'8	22'1
7	23	2	"	"	21'9	0'0	0'8	22'7
Aug. 10	Kew	1	"	"	17'4	+0'3	0'8	18'5
10	33	1	"	"	18'3	0'3	0'8	19'4
15	33	2	"	"	21'8	0'3	0'8	22'9
15	Kew	2	"	"	17'2	0'3	0'8	18'3
22	34	1	"	"	25'3	0'3	0'8	26'4
22	Kew	2	"	"	20'6	0'3	0'8	21'7
22	Kew	1	"	"	22'6	0'3	0'8	23'7
22	34	2	"	"	21'9	0'3	+0'8	23'0
Sept. 12	34	2	"	"	21'9	0'6	0'0	22'5
12	34	1	"	"	20'4	0'6	0'0	21'0
21	33	2	"	"	23'6	0'6	0'0	24'2
21	33	1	"	"	22'2	0'6	0'0	22'8
Oct. 29	33	1	"	"	25'6	0'9	0'0	26'5
29	34	2	"	"	24'1	0'9	0'0	25'0
29	34	1	"	"	22'8	0'9	0'0	23'7
29	33	2	"	"	23'7	0'9	0'0	24'6
Nov. 17	33	1	"	"	23'9	1'0	-0'8	24'1
17	34	1	"	"	22'1	1'0	0'8	22'3
17	34	2	"	"	22'4	1'0	0'8	22'6
17	33	2	"	"	24'3	1'0	0'8	24'5
17	34	1	"	Mr. Magrath.	21'0	1'0	0'8	21'2
17	33	1	"	"	24'3	1'0	0'8	24'5
17	33	2	"	"	24'3	1'0	0'8	24'5
17	34	2	"	"	21'2	1'0	0'8	21'4
18	34	1	"	"	20'5	1'0	0'8	20'7
18	34	1	"	Mr. Chambers.	20'1	1'0	0'8	20'3
Dec. 21	34	1	"	"	20'2	1'2	0'8	20'6
21	34	2	"	"	68 21'4	+1'2	-0'8	68 21'8
July 1, 1859, Mean of 96 observations ..... 68 21'5								

TABLE III.—Observations of the Magnetic Dip at the Kew Observatory in 1860.

Date.	Circle.	Needle.	Azimuth.	Observer.	Observed Dip.	Reduction to Epoch.	Correction for Annual Variation.	Corrected Dip, July 1, 1860.
1860.								
Jan. 20	33	1	0° & 180°	Mr. Chambers.	68° 21'9"	-1'2"	-0'8"	68° 19'9"
20	33	2	"	"	22'9"	1'2"	0'8"	20'9"
Feb. 17	33	1	"	"	20'7"	1'0"	0'8"	18'9"
17	33	2	"	"	21'5"	1'0"	-0'8"	19'7"
Mar. 16	33	1	"	"	20'8"	0'8"	0'0"	20'0"
17	33	2	"	"	21'2"	0'8"	0'0"	20'4"
April 2	33	1	"	Lt. Goodall, R.E.	21'9"	0'7"	0'0"	21'2"
2	33	2	"	"	24'7"	0'7"	0'0"	24'0"
18	33	2	"	Mr. Chambers.	22'8"	0'6"	0'0"	22'2"
18	33	1	"	"	21'6"	0'6"	0'0"	21'0"
26	36	1	"	"	17'3"	0'5"	0'0"	16'8"
27	36	1	"	Mr. Stewart.	18'5"	0'5"	0'0"	18'0"
27	36	2	"	"	18'6"	0'5"	0'0"	18'1"
27	35	2	"	"	20'4"	0'5"	0'0"	19'9"
27	35	2	"	Mr. Chambers.	20'6"	0'5"	0'0"	20'1"
27	36	2	"	"	14'8"	0'5"	0'0"	14'3"
28	35	1	"	"	23'9"	0'5"	0'0"	23'4"
May 2	36	1	"	Mr. Stewart.	18'1"	0'5"	+0'8"	18'4"
15	35	1	"	"	20'5"	0'3"	0'8"	21'0"
15	33	1	"	Mr. Chambers.	19'5"	0'3"	0'8"	20'0"
15	33	2	"	"	20'5"	0'3"	0'8"	21'0"
22	35	1	"	Dr. Bergsma.	19'2"	0'3"	0'8"	19'7"
22	35	2	"	"	19'0"	0'3"	0'8"	19'5"
23	36	1	"	"	18'0"	0'3"	0'8"	18'5"
24	36	2	"	"	19'7"	0'3"	0'8"	20'2"
June 18	33	2	"	Mr. Chambers.	20'1"	0'1"	0'8"	20'8"
18	33	1	"	"	19'1"	0'1"	0'8"	19'8"
19	36	1	"	Dr. Bergsma.	16'6"	0'1"	0'8"	17'3"
20	35	1	"	"	17'7"	0'1"	0'8"	18'4"
21	35	1	"	"	19'3"	0'1"	0'8"	20'0"
21	35	1	"	"	18'8"	0'1"	0'8"	19'5"
22	35	2	"	"	19'2"	0'1"	0'8"	19'9"
22	35	2	"	"	18'8"	-0'1"	0'8"	19'5"
25	35	1	"	"	23'2"	0'0"	0'8"	24'0"
25	35	1	"	"	22'3"	0'0"	0'8"	23'1"
26	35	2	"	"	18'4"	0'0"	0'8"	19'3"
26	35	2	"	"	17'8"	0'0"	0'8"	18'6"
29	36	2	"	"	19'9"	0'0"	0'8"	20'7"
29	36	2	"	"	15'1"	0'0"	0'8"	15'9"
30	36	1	"	"	19'5"	0'0"	0'8"	20'3"
30	36	1	"	"	19'7"	0'0"	0'8"	20'5"
July 2	35	1	"	"	21'7"	0'0"	0'8"	22'5"
2	36	1	"	"	22'6"	0'0"	0'8"	23'4"
3	36	2	"	"	17'7"	0'0"	0'8"	18'5"
5	36	2	"	"	18'2"	0'0"	0'8"	19'0"
5	35	2	"	"	22'9"	0'0"	0'8"	23'7"
6	36	2	"	"	15'4"	0'0"	0'8"	16'2"
7	36	2	"	"	68° 15'7"	+0'1"	+0'8"	68° 16'6"

TABLE III. (continued.)

Date.	Cycle.	Needle.	Aimath.	Observer.	Observed Dip.	Reduction to Epoch.	Correction for Annual Variation.	Corrected Dip, July 1, 1866.
1860.								
July 9	35	1	0° & 18°	Dr. Bergama.	68° 20'4	+0'1	+0'8	68° 21'3
9	35	2	"	"	17'0	0'1	0'8	17'9
9	36	1	"	"	14'5	0'1	0'8	15'4
9	36	2	"	"	13'2	0'1	0'8	14'1
23	33	1	"	Mr. Chambers.	18'6	0'2	0'8	19'6
23	33	2	"	"	21'5	0'2	0'8	22'5
Aug. 16	33	1	"	"	16'4	0'4	0'8	17'6
16	33	2	"	"	16'9	0'4	+0'8	18'1
Sept. 14	33	2	"	"	19'9	0'6	0'0	20'5
14	33	1	"	"	18'9	0'6	0'0	19'5
Oct. 19	33	2	"	"	21'2	0'9	0'0	22'1
19	33	1	"	"	20'5	0'9	0'0	21'4
22	30	1	"	Mr. Stewart.	18'0	0'9	0'0	18'9
22	30	2	"	"	20'6	0'9	0'0	21'5
22	30	2	"	"	21'2	0'9	0'0	22'1
22	30	1	"	"	14'9	0'9	0'0	15'8
23	30	1	"	"	17'6	0'9	0'0	18'5
29	30	1	"	"	19'2	0'9	0'0	20'1
30	30	2	"	"	23'0	0'9	0'0	23'9
Nov. 24	33	1	"	Mr. Chambers.	20'3	1'0	-0'8	20'5
26	33	2	"	"	21'3	1'0	0'8	21'5
Dec. 18	33	2	"	"	19'1	1'2	0'8	19'5
19	33	1	"	"	68° 18'0	+1'2	-0'8	68° 18'4
July 1, 1860, Mean of 71 observations.....								68° 19'8

*Correction for Annual Variation.*—Wherever, in the middle latitudes of the northern hemisphere, observations of the dip have been made with sufficient care, it has been found that, after elimination of the effects of secular change, the north dip is somewhat greater in winter than in summer. In the 3rd volume of the Toronto Observations, pp. cxxii and cxxiii, the following Table is given as the result of fifteen years of careful observation made throughout at the same spot and according to the same method of observation, comprising 1920 independent determinations nearly equally distributed in the different months, and averaging about 128 determinations for each of the twelve months; by combining the months equidistant from July (or the middle of the year), the influence of secular change is eliminated:—

Mean of January and the following December . . . .	$75^{\circ} 18' 90''$ N.
Mean of February and the following November ..	$75^{\circ} 18' 98''$ „
Mean of March and the following October .....	$75^{\circ} 18' 63''$ „
Mean of April and the following September .....	$75^{\circ} 18' 71''$ „
Mean of May and the following August .....	$75^{\circ} 17' 70''$ „
Mean of June and the following July .....	$75^{\circ} 17' 25''$ „

Hence on the 1st of July the mean dip at Toronto would be derived as follows, viz. :—

From the four winter months, November to February ..  $75^{\circ} 18' 97''$

From the four summer months, May to August ..... $75^{\circ} 17' 47''$ |

Showing an excess of  $1' 5''$  in the winter months above the summer months.

The annual variation at Kew, as it may be derived from the 282 determinations in Tables I., II. and III., does not differ materially from this conclusion. There are in these Tables 87 results obtained in the four winter months of the different years, and 93 results obtained in the four summer months of those years. If we collect into separate means the results in the winter months of 1857–58, 1858–59, and 1859–60, numbering them (1), (3), and (5),—and into separate means the results in the summer months of 1858, 1859 and 1860, numbering them (2), (4), and (6),—and if we compare (1) and (3) with (2), (3) and (5) with (4), (2) and (4) with (3), and (4) and (6) with (5), (by which comparisons the effects of secular change are eliminated), we find an excess of  $1' 7''$  in the mean dip of the winter months over that obtained from the summer months. The mean of the two corrections, thus separately obtained at Toronto and Kew, is  $1' 6''$ ; of which the half, or  $0' 8''$ , has been applied in the Tables with the — sign to the results in the winter months, and with the + sign to the results in the summer months.

*Probable error of a single determination of the Dip.*—It may be desirable to state the probable error of a single determination as it may be derived from the observations in the Tables, before and after the application of the correction for annual variation. It will be seen that the probable error is diminished by the application that has been made of a correction on this account,

	When uncorrected for annual variation.	When corrected for annual variation.
From the 115 Results in Table I. . .	$\pm 1.50$ . . . . .	$\pm 1.49$
From the 96 Results in Table II. . .	$\pm 1.44$ . . . . .	$\pm 1.39$
From the 71 Results in Table III. . .	$\pm 1.57$ . . . . .	$\pm 1.46$
	<hr/> $\pm 1.50$ . . . . .	<hr/> $\pm 1.45$

The probable error thus obtained represents all the diversities ascribable to the employment of different instruments (all of the one construction),—to the supposed peculiarities of different observers,—to the occasional presence of magnetic disturbance (for which no correction has been attempted),—and to differences due to different *hours* of observation;—in addition to what may be more strictly viewed as “observational errors.” It may thus serve in some measure as a guide to those engaged in similar researches, as to the degree of accuracy which is attainable in such experimental inquiries, when proper care is taken in the procurement of a reliable inclinometer and in its manipulation.

For the purpose of comparing the probable error thus obtained with inclinometers of the later English pattern with that of the instruments of earlier construction, four of the latter were selected, viz. a 9-inch circle by Robinson, a 9-inch circle by Barrow, and two 6-inch circles by Robinson, all in good order. Each circle was furnished with two needles of the same length as the diameter of the circle, and read by a lens in lieu of verniers and microscopes. Table IV. contains the particulars of 20 determinations made with these instruments in 1860 by Messrs. Stewart and Chambers. Their mean result is  $68^{\circ} 20' 04$  reduced to the epoch 1860.5, and corrected for annual variation. The mean result of the 71 determinations at the same epoch in Table III. is  $68^{\circ} 19' 8$ . There is therefore no notable difference in the mean results obtained by the two classes of instruments; but there is a considerable difference in the probable error; as from the 20 determinations in Table IV. we obtain  $\pm 3' 65$  as the probable error of a single determination with the instruments of the earlier pattern, whilst  $\pm 1' 5$  has been shown to be the probable error when inclinometers of the more recent pattern were employed.

TABLE IV.—Observations of the Magnetic Dip, at the Kew Observatory, in 1860, with 9- and 6-inch Circles (Robinson's and Barrow's), without Verniers.

Date.	Circle.	Needle.	Observer.	Observed Dip.	Reduction to Epoch.	Correction for Annual Variation.	Corrected Dip, July 1, 1860.
1860.							
Mar. 16	Robinson's 9-inch	1	Mr. Stewart.	68° 29'4	-0'8	0'0	68° 28'6
17	"	1	"	24'7	0'8	0'0	23'9
17	"	1	"	18'0	0'8	0'0	17'2
19	"	2	"	14'2	0'8	0'0	13'4
19	"	2	"	13'5	0'8	0'0	12'7
19	"	2	"	22'7	0'8	0'0	21'9
21	Barrow's 9-inch	1	"	11'0	0'8	0'0	10'2
21	"	1	"	16'5	0'8	0'0	15'7
21	"	2	"	24'0	0'8	0'0	23'2
21	Robinson's 9-inch	1	Mr. Chambers.	16'6	0'8	0'0	15'8
21	"	2	"	21'8	0'8	0'0	21'0
22	Barrow's 9-inch	2	Mr. Stewart.	26'5	0'8	0'0	25'7
22	"	2	Mr. Chambers.	23'2	0'8	0'0	22'4
22	"	1	"	18'0	0'8	0'0	17'2
May 2	Robinson's 6-inch, No. 1	1	"	26'3	0'5	+0'8	26'6
4	"	2	"	30'7	0'5	+0'8	31'0
4	Robinson's 6-inch, No. 2	1	"	18'1	0'5	+0'8	18'4
4	"	2	"	17'5	-0'5	+0'8	17'8
Nov. 26	"	1	Mr. Stewart.	19'0	+1'1	-0'8	19'3
26	"	2	"	68 18'5	+1'1	-0'8	68 18'8
July 1, 1860, mean of 20 observations .....							68 20'04

The observations were all made in the plane of the magnetic meridian.

The values obtained for the Dip at the epochs of 1821 and 1854, having been derived from observations made at the close of August and beginning of September in those years, require a small correction for annual variation, to bring them into strict comparison with the values at the two other epochs of 1838 and 1859, which have been derived from observations distributed generally throughout the years. A correction of +0'5 has been applied to each on this account.

*Corrected Dip at the several Epochs.*—We have then the observed Dips, finally corrected, at the several epochs as follows:—

1821.65.....	70° 03'4 N.....	(1)
1838.3 .....	69 17'3 .....	(2)
1854.65.....	68 31'6 .....	(3)
1859.5 .....	68 21'5 .....	(4)



Between No. 1 and No. 4 we have an interval of 37.85 years, and a mean annual secular change of  $-2'.69$ ; mean epoch, 1840.6.

Between No. 1 and No. 2, comprising an interval of 16.65 years, we have a mean secular change of  $-2'.77$ ; mean epoch, 1830.0.

Between No. 2 and No. 4, comprising an interval of 21.2 years, we have a mean secular change of  $-2'.63$ ; mean epoch, 1848.9.

Hence we may infer that the yearly diminution of the Dip from secular change, though very nearly uniform throughout the whole interval of 37.85 years, was somewhat greater in the earlier part of the interval than in the later; and that the rate of diminution may admit of being more exactly represented by the introduction of a second term.

If then we take the year 1840.0 as a convenient middle epoch  $=t_0$ , and call its dip  $\theta_0$ ; and if we further call the observed dip at the several observational epochs  $t_1, t_2, t_3$  and  $t_4$ , respectively  $\theta_1, \theta_2, \theta_3, \theta_4$ , we shall have four equations of the form

$$\theta_1 = \theta_0 + x(t_1 - t_0) + y(t_1 - t_0)^2;$$

and giving double weight to the equation furnished by the epoch 1859.5, inasmuch as it is derived from so much greater a body of observations than the results at the other three epochs, we obtain by least squares,

$$\theta_0 = 69^\circ 11'.95; \quad x = -2'.713; \quad y = +0'.00057.$$

Hence we have the general formula for computing the dip between the years 1820 and 1860,

$$\theta = 69^\circ 11'.95 - 2'.713 (t - t_0) + 0'.00057 (t - t_0)^2,$$

$t_0$  being 1840.0, and  $t$  being any other time for which the dip  $\theta$  is required.

Using this formula, we have the differences between the computed and the observed dips at the several epochs of observation as follows:—

	Computed.	Observed.	Computed—Observed.
1821.65 .....	70 03.6	70 03.4	+0.2
1838.3 .....	69 16.8	69 17.3	-0.5
1854.65 .....	68 33.4	68 31.6	+1.8
1859.5 .....	68 21.2	68 21.5	-0.3

And the dips corresponding to every tenth year within the period specified are as follows:—

1820.0	.....	70 07.3
1830.0	.....	69 39.6
1840.0	.....	69 11.9
1850.0	.....	68 45.9
1860.0	.....	68 19.9

The progressive diminution of the Dip in London during the last forty years has thus been traced and followed by the observations recorded and discussed in this paper; and the further progress of the research will now devolve on the systematic observations which are made for that purpose monthly at the Observatory at Kew.

The rate of diminution in the last forty years does not appear to differ materially from the mean rate in the preceding hundred years. The experiments of Mr. George Graham between March and May, 1723, recorded in the Philosophical Transactions for 1725, No. 389, give a mean dip in London at that epoch of "nearly"  $74^{\circ} 40'$ . Comparing this with  $69^{\circ} 11'.95$  in 1840.0, we have a difference of  $5^{\circ} 28'.1$  in 116.7 years, equivalent to a uniform diminution of  $2'.81$  annually; or if the formula

$$\theta = 69^{\circ} 11'.95 - 2'.713 (t - t_0) + 0'.00056 (t - t_0)^2$$

be employed, it gives the dip in March 1723.3 equal to  $74^{\circ} 36'.1$ , being a difference of less than  $4'$  from the result of Mr. Graham's experiments; which difference is doubtless less than the probable error of that gentleman's determination with the instruments then in use.

An expectation appears to have prevailed in some quarters that the decrease of the Dip in London should have ceased, and its subsequent increase have commenced, contemporaneously with the alteration which took place in the secular change of the *Declination* in the early part of this century, when the increase of west declination, which had been continuous in the British Islands for about two centuries, ceased, and was succeeded by a decrease of the same. But this supposition is by no means in accordance with that general view and interpretation of the phenomena of terrestrial magnetism for which we are indebted to Dr. Halley, and which, since its promulgation in 1683, has received so much confirmation in various and distant parts of the globe. In accordance with that hypothesis, the

diminution of the Dip in London might be expected to continue until the epoch should arrive when, by the easterly movement of translation of the minor magnetic system in the northern hemisphere, the disparity of the magnetic force prevailing in the European and American portions of the hemisphere should have attained its maximum:—which is certainly not yet the case.

Is there then, in the secular change of the Dip, no feature in which, in conformity with the Halleian hypothesis, an alteration might be expected to synchronize with the reversal in the direction of the secular change of the declination? Assuredly there is; and the facts which recent investigations have brought to our knowledge manifest that such an alteration has taken place. I proceed to describe it.

If we have recourse to those extensive generalizations which, under the name of “Isoclinal Lines corresponding to particular Epochs,” present a connected view of the changes which have taken place from time to time in the magnetic lines of the Dip over large portions of the earth’s surface, and enable us to anticipate with some degree of confidence the changes which may be expected to take place in years to come, we notice generally that the lines undergo two species of modification, or peculiarities of change, which it is necessary to keep separately and distinctly in view. In the British Islands, for example, the Isoclinal Lines for little less than two centuries past have been steadily advancing towards the north by a gradual movement of translation. This is one feature of the secular change; but there is a second feature, which, if not at first sight equally striking, is yet equally regular and systematic in its operation; viz. the *direction* of the isoclinal lines as they pass across our country from the south-west towards the north-east undergoes a small but sensible change from year to year, by which, in the lapse of several years, the angle at which they cut the geographical meridians is materially altered. By the joint operation of these two processes, the *general configuration of the lines* over large portions of the earth’s surface, as well as *their values in particular localities*, are both subject to systematic alteration; a remark which is not limited to the isoclinal lines alone, but is the case also in the isogonic and isodynamic lines. Those who are conversant with Dr. Halley’s writings, will be aware that,—in correspondence with his views,—between the epochs when the Dip in London should

attain, respectively, the maximum and the minimum amount which constitute its limits under the system of secular change, an *intermediate* epoch might be anticipated, when the isoclinal lines passing across the British Islands should attain their least angle of inclination to the geographical meridian ; towards which they should have progressively advanced, and from which they would as progressively recede. Now, if we compare the line of  $70^{\circ}$  of dip in the Isoclinal Map of 1780 of the *Magnetismus der Erde* with that of 1840 in Mr. Keith Johnstone's Physical Atlas, plate 23, we may fix on a point in about  $42^{\circ}$  North Latitude and  $30^{\circ}$  West Longitude, in which the Dip has remained nearly stationary, and through which the line of  $70^{\circ}$  of Dip passed, at both epochs ; and we may perceive that, in its easterly course from that point or pivot, this line passed in 1780 *through the middle of France considerably to the South of Paris* (where the Dip was then between  $71^{\circ}$  and  $72^{\circ}$ ) ; whereas in 1840 it passed *across England considerably to the north of London* (where the Dip had diminished to little more than  $69^{\circ}$ ). Therefore in the sixty years which had elapsed between the two epochs, 1780 and 1840, the direction of the lines as they impinged upon Western Europe had become much less inclined to the geographical meridian (*i. e.* forming a greater angle with the parallels of latitude) in 1840 than in 1780 : and if we consult still earlier maps, we find that a change in the same direction had been progressive from a still earlier period. The particular year in which this feature attained its limit, and an opposite change commenced, cannot now perhaps be precisely determined ; it was probably somewhat earlier than 1840. But from the comparison of the magnetic surveys of the British Islands in 1836–37 and 1857–58, it is certain that the change in the direction of the isoclinal lines in this part of the globe has entered upon the contrary phase to that which had previously existed. The observations of the late Mr. Welsh in Scotland in 1857–58 (*Brit. Assoc. Reports*, 1859), when compared with those of the Scotch Survey made in 1836–37, published in the British Association Reports for 1836, show, according to Mr. Balfour Stewart's calculation, that an increase of several degrees in the angle at which the lines cut the meridians in passing across Scotland has taken place between the epochs of the earlier and the later surveys. The same general conclusion follows from a comparison of the magnetic surveys of England at nearly the same epochs ; every-

where near the west coast of England the mean annual secular change in the twenty years has been greater, and near the east coast less than its mean value at Kew; showing that the general direction of the isoclinical lines more nearly approaches a parallelism to the equator now than it did twenty years ago. The ascertainment of the exact value of the secular change at a particular locality by a well-conducted system of periodical observations is the duty of a magnetic *observatory*; the direction of the magnetic lines passing across a country is supplied by magnetic *surveys*; which, for that purpose, ought to be repeated from time to time, as they have now been in this country, at intervals of perhaps twenty or twenty-five years.

It has been imagined that the secular changes of the magnetic elements may be due to some alteration taking place either in the distribution or in the condition of the materials in the interior of the globe. But the regularity and uniformity with which the secular magnetic changes continue through long intervals of time, together with their sudden periodic reversals,—and their corresponding features in the northern and southern hemispheres, which add greatly to the apparent consistency and systematic character of the whole as parts of a uniform general system,—wear more the aspect of effects of some yet unascertained *cosmical* cause. One of the British Colonial Observatories, St. Helena, having the advantage of both a large secular change and a small amount of magnetic disturbance, has afforded a very striking example of the great regularity with which the secular change takes place, maintaining a steady uniformity, traceable not only from year to year, but from month to month, and even from week to week; so that it is not too much to say that, from observations made during a single fortnight, an annual secular change which has existed almost without variation for more than a century, may be ascertained and measured with very considerable precision. (Magnetic Observations at St. Helena, vol. ii. p. ix.)

*March 21, 1861.*

Major-General SABINE, R.A., Treasurer and Vice-President,  
in the Chair.

Professor Herman Helmholtz, elected a Foreign Member, was admitted into the Society.

The following communications were read :—

- I. "On the Relations of the Vomer, Ethmoid, and Intermaxillary Bones." By JOHN CLELAND, M.D., Demonstrator of Anatomy in the University of Edinburgh. Communicated by Professor HUXLEY. Received February 28, 1861.

(Abstract.)

The author commences by describing the connexions of the vomer in mammalia generally, and states that of these by far the most constant in occurrence and invariable in its nature is one to which he believes he has been the first to attract attention, viz. that by which it comes in contact with the lateral masses of the ethmoid. He states that this connexion is effected by the alæ of the vomer being continued into two laminæ which pass beneath the turbinate of the ethmoid, and are united to the framework of the lateral masses at a point corresponding to the margins of the sphenopalatine foramina. These laminæ he calls the "ethmovomerine laminæ," and for the sphenopalatine foramen he adopts the term "nasal foramen of the palate-bone," because it is not the sphenoid, but the ethmoid, which completes that foramen in cases where it is not quite encircled by the palate-bone.

According to the author, the connexion of the vomer, next in importance to that with the lateral masses of the ethmoid, is that with the intermaxillaries, which is constantly found when the mesial palatine processes of the intermaxillaries are developed, and is always of such a description that the groove formed by the superior margins of these processes is continuous with the vomerine groove, while their inferior margins likewise form a continuous line with the inferior margin of the vomer.

As to the connexion of the vomer with the central plate of the ethmoid, the author considers it altogether of secondary importance; for, while that with the lateral masses of the ethmoid takes place at a very early date, and is a connexion of continuity, *i. e.* by the bones lying edge to edge, that with the central plate is of late occurrence, and is one of contiguity, *i. e.* by the surfaces of the bones coming accidentally in contact. The connexions of the vomer with the maxillaries and palate-bones are also considered to be of secondary importance.

In reference to the peculiarities presented by the vomer and ethmoid in the human subject, the author endeavours to show that their relations are really in agreement with those which he has described as typical among mammalia. He found the key to the interpretation of the arrangement seen in the adult, by examining the condition of the bones in very early life, especially the sphenoidal spongy bones. The last-mentioned bones are described in young subjects as consisting of hollow pyramids enclosing the sphenoidal sinuses, but originally quite unconnected with the sphenoid. They articulate on the one hand, edge to edge with the alæ of the vomer, and on the other with the ethmoid and the palatals, and by their contact with the orbital and sphenoidal processes of the latter, complete on either side the so-called sphenopalatine foramen, and correspond altogether to the ethmovomerine laminæ and part of the ethmoid in other mammals.

The human vomer can be seen in its perfection as a separate bone, and with all its connexions fully developed, in early life only; and an explanation is offered of the peculiarities of the human vomer and ethmoid, by showing that they are consequences of the great curvature of the cranial arch, the small development of the organ of smell, and the prolongation of the face downwards for the sake of voice, instead of forwards as in other animals, for the sake of prehension.

The connexions of the vomer, ethmoid, and intermaxillaries in the various families of mammals are next examined in detail, and various differences of form, &c. peculiar to genera and orders are noticed. Attention is particularly drawn to the circumstance that in the camels and other ruminant families the intermaxillaries are prolonged forwards beyond the points of junction of their lateral plates and mesial palatine processes, so as to embrace more or less completely the anterior extremity of the septal cartilage of the nose, which between these prolongations dips into the palate; and that, similarly, the intermaxillaries meet above the septal cartilage in the Dugong, Manati, and Tapir, in which they have no mesial palatine processes: to these forms importance is afterwards attached in comparing the intermaxillaries of mammals with those of birds, reptiles, and fishes.

The author argues that the vomer, lateral masses of the ethmoid, and palate-bones are members of one segment; that the central plate of the ethmoid is the centrum of the segment behind; and that the mesial palatine processes of the intermaxillaries play the part of cen-

trum to the segment in front of the vomer. He considers the skull to be composed of seven segments; that three of these are developed for protection of the encephalon, viz. posteriorly the occipital segment, then the parietal segment, to which belong the postsphenoid and parietals, and anteriorly the frontal segment, to which the frontal and central plate of the ethmoid belong; and that three other segments with imperfect neural arches, and each connected with a special sense, alternate with the first three, viz. the auditory, optic, and olfactory segments, the last-mentioned being the vomerine segment; and that foremost of all is a terminal seventh segment, the facial segment, to which the intermaxillaries, maxillaries, and nasals belong.

With a special view towards determining the exact construction of the facial segment, he examines the anterior portions of the skulls of birds, reptiles, and fishes. Taking as his guide the constitution of the vomerine segment as he believes he has established it in mammals, he concludes that the vomer in birds, reptiles, and fishes is the bone described as such by Professor Owen; that the lateral masses of the ethmoid are the prefrontals of reptiles and fishes, and are absent from the skull of birds; that the central plate of the ethmoid is represented by the interorbital septum (whether ossified or not) of birds, reptiles, and fishes; and that in fishes the nasals are represented by the nasal of Owen, which, however, usually contains an additional element, which plays the part of centrum, and corresponds to the mesial palatine processes of the intermaxillaries of other classes. His examination of the skulls of birds, reptiles, and fishes has convinced him that throughout the vertebrata the lateral plates of the intermaxillaries form the proximal part of a hæmal arch, of which the maxillaries compose the distal part, while the nasals are elements of the neural arch of the same segment; that thus in mammals the ring of bone surrounding the incisive foramina is the first hæmal arch; that the nostrils in all vertebrata are openings situated behind the facial and in front of the vomerine segment; and that the alar cartilages of the nose are structures serially homologous with the tarsal cartilages and the pinnæ of the ears.

In attempting to explain the exact morphological relations of the nasals, the author enters into the embryological bearings of the conclusions at which he has arrived, and puts forward the hypothesis



that the anterior segments of the skull tend to undergo fission in the direction of the axis of the chain of segments, and that the cleft separating the maxillary lobe of the embryo from the middle and lateral frontal lobes is morphologically horizontal—lying between the hæmal and neural elements of more than one segment; and in proof of this view a variety of evidence is adduced.

## II, "On the Structure and Growth of the Tooth of *Echinus*."

By S. JAMES A. SALTER, M.B. Lond., F.L.S., F.G.S.

Communicated by THOMAS BELL, Esq. Received March 5, 1861.

(Abstract.)

The author commences his paper by stating that the researches upon which it is based were made more than four years since, and then without the knowledge that the structure had been previously investigated by others.

An abstract of the *literature* of the subject (contained in very narrow limits) is then given.

In 1841 Valentin, in Agassiz's Monograph on the Echinoderms (*Anatomie des Echinodermes*), published a description and many good figures of the minute anatomy and growth of the Echinus-tooth.

Professor Quekett, in his 'Lectures on Histology' (1854), referring to the minute *mature* anatomy of the organ, states its ultimate structure to resemble bone and dentine of vertebrata.

Dr. Carpenter, in his work 'On the Microscope,' speaks of the tissue of the tooth as essentially of the same nature as the shell of the Echinidæ generally (1856).

Lastly, Professor W.C. Williamson describes the subject more fully than his predecessors, entering into the question of the development of the tooth both generally and histologically (though apparently in ignorance of Valentin's Essay), in a paper on the "Histology of the Dermal Tissues," &c., in the British Journal of Dental Science, 1857.

The coarse anatomy and relations of the Echinus-tooth are then described, and the question is discussed as to how far the organ resembles and how far it does not resemble the incisor tooth of a Rodent mammal, to which it has constantly been likened.

Some remarks then follow on the method of investigation, which the peculiar physical characters of the structure render very difficult.

Before describing the histology of the mature tooth, the author premises some succinct remarks upon the several elementary parts that are formed at its growing extremity, and by which its complex structure is built up—showing how the shape and plan of these elements determine the microscopical appearances of the several regions of the tooth as seen in different sections.

These elementary parts are—(1st) the *Primary plates*, which consist of a double series of triangular sheets of calcareous matter, and which constitute the physiological axis of the tooth, about which and connected with which the four secondary elements are developed. These latter are (2nd) the *Secondary plates*, lappets of similar calcareous sheets attached to the outer edge of the primary plates; (3rd) the *Flabelliform processes*, elaborate reticulations of calcareous fibres ending in fan-shaped extremities; (4th) the *Keel fibres*, certain long cylindrical rods with club-shaped ends of the same chemical nature, which pass towards the enteric region of the tooth in their growth; and (5th) the *Enamel Rods*, which are minute very short developments of the same character, and which are formed in the opposite direction. Thus far a primary and secondary stage of formation are represented: a third stage, that of consolidation, now occurs in the development of (6th) the *Soldering particles*, multitudes of minute discs of carbonate of lime which appear over the whole surface of the previously-formed elementary parts, and by which they are soldered together, the intervals between these (in a certain sense) constituting the tubular character of the mature tissue.

The primary plates, secondary plates, and the proximal portion of the flabelliform processes are stated to constitute the body of the tooth—the distal extremities of the flabelliform processes the *skirtings* of the enteric region of the body of the tooth; the keel fibres wholly form the keel; while the short enamel rods compose the thin white layer on the dorsal surface of the tooth—the enamel.

The histology of the tooth is remarkable as exhibiting apparent inconsistencies in different lines of section. A vertical section of the tooth presents the appearance of vertebrate bone, lacunæ, canaliculi, and lamellæ; while a transverse section displays some regions resembling dentine (the body of the tooth), and others having the closest

similitude to an oblique section of the shell of some Mollusca, such as *Pinna*.

The author then proceeds to describe in detail and with particularity the form and progressive growth of the several elements of the tooth as they are met with in examining the growing extremity and proceeding from it towards the mature structure, as long as the elements are susceptible of isolation and individual examination. The anatomy of the soldering particles, and their relation to the production of the cavitory structure of the tooth, is specially dwelt upon. The soldering particles are supposed to be isolated at first, but as they enlarge they become connected by a thin film from their upper and under faces. This occurs before the final consolidation of the tissue, and before the soldering particles are indissolubly connected with, and themselves indissolubly connect, the contiguous elements of the tooth. At this stage these particles are still susceptible of isolation, and they may be separated *en masse*, being held in relative position by the films that connect them. The soldering particles and the connecting films thus constitute a tubular system, which has an independent existence before the final consolidation of the tissue, and this tubular system is introduced between, and interpolated among the previously existing elementary parts of the tooth.

The author concludes by expressing a coincidence of opinion with Dr. Carpenter, that the minute structure of the tooth is essentially of the same nature as that of the shell of the Echinidæ generally.

*April 11, 1861.*

Major-General SABINE, R.A., Treasurer and Vice-President,  
in the Chair.

The following communications were read :—

- I. "On the Motion of a Plate of Metal on an Inclined Plane, when dilated and contracted ; and on the Descent of Glaciers." By the Rev. HENRY MOSELEY, M.A., Canon of Bristol, F.R.S., Inst. Sc. Paris Corresp. Received March 14, 1861.

(Abstract.)

The case in which the upper edge of such a plate (supposed rec-

tangular) is fixed is first discussed; and then that in which the lower edge is fixed. Each of these cases is considered subject to the condition of friction; first, when the plate is dilated, and secondly, when it is contracted. Two other principal conditions arise in the discussion; one being that in which a part only, and the other that in which the whole of the plate dilates and contracts.

In the former the dilatation or contraction is represented by

$$\frac{E \lambda^2 t^2 \cos \phi}{2\mu(1 \pm \lambda t) \sin(\phi \pm i)},$$

or by

$$\frac{E \lambda^2 t^2 \cos \phi}{2\mu(1 \mp \lambda t) \sin(\phi \mp i)},$$

according as the plate is fixed at the top or the bottom.

In the latter it is represented under the same conditions by

$$a \left\{ \lambda t - \frac{\mu \sin(\phi \pm i)}{2E \cos \phi} a \right\}$$

or by

$$a \left\{ \lambda t - \frac{\mu \sin(\phi \mp i)}{2E \cos \phi} a \right\}.$$

In which formulæ—

$a$  represents the length of the plate.

$\mu$  its weight in lbs. per foot of its length.

$i$  the inclination of the plane.

$\phi$  the limiting angle of resistance (the angle of friction) between the surface of the plane and of the plate.

$E$  the modulus of elasticity of the plate.

$\lambda$  the dilatation or contraction per foot of the length for each variation of  $1^\circ$  of Fahrenheit.

$\pm t^\circ$  the rise or fall of the temperature in degrees of Fahrenheit, by which the dilatation or contraction of the plate is supposed to be caused.

In the case in which no part of the plate is fixed a horizontal line may be taken in it above which it *dilates* upwards, and below it downwards. The position of this line is determined by the consideration that, if the plate be imagined to be cut through along that line, the thrust necessary to push the part above upwards must be equal to that necessary to push the part below downwards.

In like manner a horizontal line may be found above which the plate *contracts* downwards and below it, upwards.

The former neutral line is nearer the top than the bottom, the other nearer the bottom than the top. The one is at the same distance from the top as the other is from the bottom. This distance is represented by the formula

$$\frac{1}{2}a \frac{\sin(\phi - \epsilon)}{\sin \phi \cos \epsilon}.$$

When the plate is dilated, it is the longer portion which dilates *downwards*; and when it is contracted, it is the shorter portion which contracts *upwards*. The lower end of the plate descends therefore by a given increase of temperature more than it ascends by an equal fall; and on the whole the plate descends.

If we suppose the temperature first to be increased by  $t_1^\circ$ , and then diminished by  $t_2^\circ$ ; then—

1st. In the case in which a portion only of the plate dilates, the descent is represented by

$$\frac{E\lambda^2 \cos \phi}{2\mu} \left\{ \frac{t_1^2}{(1 + \lambda t_1) \sin(\phi - \epsilon)} - \frac{t_2^2}{(1 - \lambda t_2) \sin(\phi + \epsilon)} \right\}.$$

2ndly. In the case in which the whole plate dilates and the whole contracts, the descent is

$$\frac{1}{2}a \left\{ (t_1 - t_2)\lambda + (t_1 + t_2) \frac{\tan \epsilon}{\tan \phi} - \frac{\mu a \sin(\phi + \epsilon) \sin(\phi - \epsilon) \tan \epsilon}{E \sin^2 \phi \cos \epsilon} \right\}.$$

The first case passes into the second.

If  $E$  be very great as compared with  $\mu a$ , the second term in the above formula may be neglected. It then corresponds with the formula given by the author in a former communication to the Society.

To verify the fact of the descent of a plate of metal under the conditions supposed, a deal board 9 feet long and 5 inches broad, was fixed at an inclination of  $18\frac{1}{2}^\circ$  against the wall of a house having a southern aspect, and a sheet of lead was placed upon it one-eighth of an inch thick and weighing 28 lbs., and having its edges turned over the edges of the board so as not to bind upon it. Near the lower extremity a vernier was constructed, by which the position of the lead on the board could be determined to the 100th of an inch. Its position was observed daily between 7 and 8 in the morning and 6 and 7 in the evening, from the 16th of February to the 28th of June, 1858.

A Table is given showing the descent for every day of that period, from 7 A.M. to 6 P.M. and from 6 P.M. to 7 A.M.

In the months when there was no sunlight from 6 P.M. to 7 A.M., there was no descent in that interval. The descents from 7 A.M. to 6 P.M. were very different on different days. Sometimes they amounted to a quarter of an inch in the day, and sometimes were not appreciable. The greatest descents were on sunny days, and especially when with a warm sun there was a cold wind. The least were on days of continual rain. The average daily descents were, in inches,—

February.	March.	April.	May.	June.
·10000	·13806	·16133	·21500	·21888

These descents were not due to the extreme temperatures of the periods in which they took place, but to the aggregate of the variations up and down during each interval. The difference of the highest and lowest temperatures in any interval may have been small, and yet the changes of temperature up and down may have been many, and their aggregate great. It is upon this aggregate that the descent depends.

The dilatation of ice was measured in the years 1845, 1846, at the Observatory of Pultowa, by Schumacher, Pohr, and Moritz; and the particulars of their experiments were communicated to the Academy of St. Petersburg, by W. Struve, in 1848, and published in its *Memoirs* (*Sciences Mathém. et Phys., sér. 6. t. iv.*). By exposing water to the action of the frost in a mould, Schumacher obtained a block of ice, which, after reducing it with the plane, measured 6 ft. 3 ins. in length and 6 ins. by  $6\frac{1}{4}$  inches in section; and he caused three thermometers to be frozen into it with their stems projecting above its surface. This block of ice he carried out from a room, where it had been preserved at a uniform temperature of  $-2^{\circ}$  R. during the day, into the open air at night, and slung it in a horizontal position from a beam supported by tressles. As its temperature fell he measured the distance between two steel points frozen into it near its two ends, by a measuring rod of dry wood (well-clothed), the distances on which were referred to a standard measure on the wall of a room of the Observatory which retained nearly a constant tempe-

perature of  $-2^{\circ}$  R. His measurements had reference to observed temperatures of the ice varying from  $-2^{\circ}3$  R. to  $-22^{\circ}$  R. After applying the requisite corrections, it resulted from them that the coefficient of expansion of ice is for  $1^{\circ}$  R.

·00006466,

which is *nearly* twice as great as the coefficient of dilatation of lead, and *more* than twice as great as that of any other solid.

We do not know the modulus of elasticity of ice, or the pressure under which it disintegrates.

If it were as elastic as slate and did not resist crushing more than hard brick, a block of it placed with its ends between two immoveable obstacles, would crumble when its temperature was raised one degree of Fahrenheit. It is its great dilatability which gives to ice this tendency to disintegrate, when, not being free to dilate, its temperature is raised\*, even so slightly as this.

If the block of ice experimented on by Schumacher had been placed upon a plank inclined at the same angle as that used in the experiment with the lead was, and if its under side had been coated with lead-foil so as to give it the same friction on the plank as the lead had, then, under the same variations of temperature as the lead experienced, it could not but have descended as the lead did, but twice as fast, because its dilatability is twice as great.

We may conceive such a block of ice to be made up of thin plates parallel to its upper surface, such as plates of glass would be, if glass were as dilatable as ice and as friable, and if it possessed that property of passing from a disintegrated into a solid state, which in ice is called regelation. If we put the adherence of these plates to one another in the place of friction, and conceive the variations of external temperature (or the effects of solar radiation) to reach them in succession, each one being dilated or contracted independently of the rest, then each would descend by a motion proper to itself, and also by reason of the descents of those subjacent to it. The extremities of the plates would under these circumstances overlap, and the descent of each, proper to itself, would be increased by the overlappings of those beneath it.

\* Agassiz describes a disintegration of the transparent ice of the blue bands of glaciers when laid bare, which appears to be due to its expansion.—Bulletin Un. de Genève, vol. xlv. p. 142.

Each plate would under these circumstances descend faster than the one beneath ; and supposing the adherence of the lowest plate to the board to be the same as that of the plates to one another, then, of any number of blocks similarly placed and subject to the like variation of temperatures, the thickest or deepest would descend, at its surface, the fastest ; and if there were a block of different depths in different parts, the deepest parts would descend the fastest. The differential motion thus set up would not be appreciable in a block of ice of different thicknesses in different parts if its dimensions were no larger than the block experimented on by Schumacher, but in a glacier it would be appreciable.

To bring Schumacher's block to the proportions of a glacier, it must be converted into a slab twelve feet long, twenty inches wide, and two inches thick. It would then represent on a scale of the 1500th part, a glacier 2500 feet wide, 250 feet deep, and 18,000 feet long, which are something like the dimensions of the Mer de Glace from 2300 feet below Montanvert to the Tacul. If we suppose it to be placed at the same inclination of  $18\frac{1}{2}^{\circ}$  at which the lead was, its under surface being coated with lead so as to have the same friction on board as the lead had, then it may be calculated that if it had experienced the same variations of temperature as the lead did, its average daily descent, measured in inches, would have been

February.	March.	April.	May.	June.
·26666	·36816	·43022	·37334	·58368

If, now, we conceive its inclination to change from  $18\frac{1}{2}^{\circ}$  to that of the Mer de Glace, which is about  $5^{\circ}$ , and its dimensions to become actually those of that glacier, then, supposing the glacier to experience the same elevations and depressions of temperature as the lead did, its average daily descents in inches would be

February.	March.	April.	May.	June.
104·56	144·06	168·74	224·87	228·92

which rates of motion are probably twelve times greater than the actual rates of motion of the glacier ; showing that variations of the temperature of the glacier twelve times less than those of the lead, would be sufficient to produce its actual descent ; or that it would



descend as it actually does, if the resistances opposed to its descent were twelve times greater than the resistances opposed to the descent of the lead.—If its descent were resisted by a friction, for instance, having twelve times the coefficient of that of the lead on the board, or such as would cause it to rest without slipping on an incline having twelve times the tangent of the inclination of the board; or if the variations of temperature were less and the resistance greater in any proportion which would retard the descent twelve times as much. So that we may suppose in the case of the glacier a far greater resistance in proportion than that sustained by the lead upon the board, and variations of temperature far less, without passing the limits within which a probability is created by the experiment that the descent of the glacier is due to the same cause as that of the lead.

In the act of descending on the board, the slab of ice of which we have spoken could not but be thrown into a state of extension in some parts and of compression in another. The conditions of the descent being in other respects given, the amount of this extension or compression might be at any point determined. If at any point the extension exceeded the tenacity of the ice, the slab would there separate across its length; and if at any point the compression exceeded the resistance to crushing, it would there crush.

Supposing it to be thinner at the sides than in the middle, the surface-motion of the middle would be faster than that of the sides, and from this differential motion would result cracks oblique to the axis of the slab, the explanation of which, as they exist in glaciers, is one of the most successful attempts yet made at the solution of the mechanical problem of glacier-motion. These conditions of the descent of the slab, when referred to a glacier, explain the formation of transverse and lateral crevasses, and the fact of a glacier crushing itself through a gorge.

The Mer de Glace moves faster by day than by night\*. Its mean daily motion is twice as great during the six summer as during the six winter months†. It moves fastest in the hottest months, and in those months varies its motion the most, because in them the variations of temperature are the greatest. It moves most slowly in the coldest months, and in those varies its motion the least, because in those months the variations of temperature are the least. These differences

\* Forbes, 'Occasional Papers,' p. 12.

† Ibid. p. 129. Tyndall, 'Glaciers of the Alps,' p. 294.

are more remarkable at lower stations on a glacier than at higher, "because the lower are exposed to more violent alternations of heat and cold than the higher : this (says Forbes) we shall find to be general."

It moves fastest on the hottest days. "This I apprehend (says Forbes) to be clearly made out from my experiments, that thawing weather and a wet state of the ice conduce to its advancement, and that cold, whether sudden or prolonged, checks its progress \*." "The striking variations in September, especially at the lower stations, which were frequently observed, prove the connexion of temperature with velocity to a demonstration†."

It is, however, impossible to do justice to the positive character of the evidence on which this conclusion has been founded by Professor Forbes without reference to those diagrams, by means of which he has compared the mean rates of the daily motions of glaciers and the corresponding mean temperatures. This comparison is founded on observations made by himself and Aug. Balmat, as to the motion of the "Mer de Glace," at fourteen different stations in three different years, and on observations on the mean temperature of the atmosphere made at the same times at the Great St. Bernard and at Geneva. It results from it that no change in the mean temperature of the atmosphere is unaccompanied by a corresponding change in the mean motion of the glacier.

The glacier moves with different velocities at different depths, the surface-motion being faster (probably two or three times) than that of the deepest part. The motions at different depths cannot but be related to one another : so that as the influence of variations of temperature is felt on the *surface*, it cannot but be felt throughout the glacier.

If every change of solar heat is associated with a corresponding change of glacier-motion, it seems to follow that the two are either dependent upon some common cause, or that the one set of changes is caused by the other ; and the former of these conclusions being inadmissible, we are forced on the latter. It is not necessary to show how it is that changes of external temperature penetrate glaciers. Of the power of the sun upon them there are, however, evidences in the ablation of surface constantly going on and in the preservation of the ice which is covered by the stones of a moraine, which sometimes forms an icy ridge from 50 to 80 feet high, and some hundred feet in width.

\* Forbes, 'Travels in the Alps,' p. 148.

† Ibid.  
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"The sun's rays," says Tyndall\*, "striking upon the unprotected surface of the glacier, *enter the ice to a considerable depth*; and the consequence is that the ice near the surface of the glacier is always disintegrated, being cut up into minute fissures and cavities filled with water and air, which, for reasons already assigned, cause the glacier when it is clean to appear white and opaque. The ice under the moraines, on the contrary, is usually dark and transparent. I have sometimes seen it as black as pitch, the blackness being a proof of its great transparency, which prevents the reflexion of light from its interior. The ice under the moraines cannot be assailed in its depths by the solar heat, because this heat becomes obscure before it reaches the ice, and as such it lacks the power of penetrating the substance. It is also communicated in great part by way of contact instead of by radiation. A thin film at the surface of the moraine ice engages all the heat that acts upon it, its deeper portions remaining transparent and intact."

It matters not to the argument how little below freezing the temperature of a glacier may be. So long as the ice *exists in a solid state* and is capable of being penetrated by the solar heat, it cannot but dilate and contract. Its central portions, lying folded in ice 100 feet thick above and below, may well, however, be conceived to retain some of the cold of the region from which they have descended. The observations of Agassiz on the temperature of the Aar Glacier are not to be relied upon, because the access of damp external air to the borings in which they were made, and of water percolating the disintegrated ice of the surface, was not effectually stopped. The included thermometers could not but under these circumstances show zero, although the temperature of the surrounding ice was below it. For the water freezing on the walls of the boring, the latent heat thereby given out, would raise the temperature of the air about the bulb to the freezing-point, and this water being continually renewed, the quicksilver would always be kept at that point.

That glacier-ice possesses no such properties of viscosity or compressibility as would cause it to descend by its weight along such slopes as those on which some glaciers descend may be shown thus. Let the Mer de Glace be conceived to be cut up by vertical sections at right angles to one another, into blocks, whose bases are large enough to prevent them toppling over; and let these blocks be

\* *Glaciers of the Alps*, p. 294.

imagined to be separated from one another. Then (supposing it not to slip) each block would stand in its place without the support of the neighbouring blocks; for its vertical sides would be walls of ice needing no external support, like the ice-wall of the *Glacier du Géant*, 141 feet high near the *Tacul*, described by Tyndall\*; or that of the *Mer de Glace* near the *Angle*, pictured by Forbes†. Needing no external support when thus placed asunder, they could need none when brought again together; nor could they, by the fact of their being so brought together, be made to exert any mutual pressure, or have any more or other tendency to move than each block had separately. If this reasoning be true, there is no physical property of ice, whether it be called viscosity or plasticity, which would cause it to descend by its weight alone on any surface along which it would not slide. It is plastic no doubt—Tyndall has proved that by the Hydraulic Press,—but not as to any pressure created in a glacier by the weight of the glacier. If it were, or if it were semifluid, then under those enormous pressures which it is supposed to sustain, it would bulge out at the ice-wall of the *Tacul*, and mould itself to the sides of its channel; for it is the character of a compressible substance, not less than of a semifluid, to yield not only in the direction in which pressure is applied to it, but in every other.

Nor if it were sufficiently a fluid to flow by its weight alone, however slowly, down slopes of  $3^{\circ}$  or  $5^{\circ}$ , could it descend otherwise than as a torrent down slopes, such as that of the *Silberberg Glacier*, of  $40^{\circ}$ , on which its descent is nevertheless several times slower. The phenomena of these secondary glaciers offer themselves as a test of rival theories of glacier-motion. They lie on slopes so steep that it is scarcely possible to conceive the ice, if solid, to be loosened from the face of the rock, and not to descend in fragments; or if viscous, not to become a torrent.

II. "Preliminary Note on the production of Vibrations and Musical Sounds by Electrolysis." By GEORGE GORE, Esq. Communicated by Professor TYNDALL. Received April 4, 1861.

If a large quantity of electricity is made to pass through a suitable good conducting electrolyte into a small surface of pure mercury,

\* *Glaciers of the Alps*, p. 289.

† *Travels in the Alps*, p. 76.

and especially if the mercurial surface is in the form of a narrow strip about  $\frac{1}{8}$ th of an inch wide, strong vibrations occur; and symmetrical crispations of singular beauty, accompanied by definite sounds, are produced at the mutual surfaces of the liquid metal and electrolyte.

In my experiments the crispations and sounds were readily produced by taking a circular pool of mercury from 1 to 3 inches in diameter, surrounded by a ring of mercury about  $\frac{1}{8}$ th or  $\frac{1}{16}$ th of an inch wide, both being contained in a circular vessel of glass or gutta percha, covering the liquid metal to a depth of about  $\frac{1}{2}$  an inch with a rather strong aqueous solution of cyanide of potassium, connecting the pool of mercury by a platinum wire with the positive pole of a battery capable of forcing a rather large quantity of electricity through the liquid, and connecting the ring of mercury with the negative platinum wire. The ring of mercury immediately became covered with crispations or elevated sharp ridges about  $\frac{1}{16}$ th of an inch asunder, all radiating towards the centre of the vessel, and a definite or musical sound was produced capable of being heard, on some occasions, at a distance of about 40 or 50 feet. The vibrations and sounds ceased after a short time, but were always reproduced by reversing the direction of the electric current for a short time, and then restoring it to its original direction. The loudness of the sound depends greatly upon the power of the battery; if the battery was too strong the sounds did not occur. The battery I have used consists of 10 pairs of Smee's elements, each silver plate containing about 90 square inches of immersed or acting surface; and I have used with equal success six Grove's batteries, arranged either as 2 or 3 pairs, each platinum plate being 6 inches long and 4 inches wide. If the cyanide solution was too strong, the sounds were altogether prevented.

Being occupied in investigating the conditions and relations of this phenomenon with the intention of submitting a complete account of the results to the notice of the Royal Society, I refrain from stating further particulars on the present occasion.

*April 18, 1861.*

Major-General SABINE, R.A., Treasurer and Vice-President,  
in the Chair.

The following communication was read :—

“On the Effect produced on the Deviation of the Compass by the Length and Arrangement of the Compass Needles; and on a New Mode of correcting the Quadrantal Deviation.” By ARCHIBALD SMITH, Esq., M.A., F.R.S., late Fellow of Trinity College, Cambridge; and FREDERICK JOHN EVANS, Esq., R.N., Superintendent of the Compass Department of Her Majesty’s Navy. Received April 13, 1861.

(Abstract.)

When the length of the compass needle may be neglected compared with the distance of the iron which acts on a ship’s compass, the deviation is accurately expressed by the formula

$$\sin \delta = A \cos \delta + B \sin \zeta' + C \cos \zeta' + D \sin (\zeta + \zeta') + E \cos (\zeta + \zeta') ;$$

in which  $\zeta$  is the azimuth of the ship’s head measured *eastward* from the *correct magnetic north* ;

$\zeta'$  is the same azimuth, but measured from the direction of the *disturbed needle* ;

$\delta = \zeta - \zeta'$  is the *easterly* deviation of the needle ;

A, D, E are coefficients depending on the distribution of the soft iron of the ship.

B and C are coefficients depending partly on the distribution of the hard and soft iron of the ship, and partly on the magnetic dip and horizontal force at the place.

In all ships which have been examined, A and E are so small that they may be neglected ; and, if the deviation be of such an amount that we may take  $\delta$  for  $\sin \delta$ ,

$$\delta = B \sin \zeta' + C \cos \zeta' + D \sin (\zeta + \zeta') .$$

The first two terms represent the “Semicircular” deviation, the last term the “Quadrantal” deviation.

The “semicircular” deviation is, on the plan proposed by the Astronomer Royal, and extensively practised in the mercantile marine, corrected by magnets ; the “quadrantal” by masses of soft iron placed on each side and at the same level as the compass ; and when the distance of the correction is sufficiently great, this correction may be considered as perfect for the time and place at which it is made ; but when this is not the case, errors are introduced, which it is the object of the paper to consider.

Mr. Evans observed that the standard compass of the ‘Great

Eastern,' which had been corrected on Mr. Airy's plan by Mr. Gray of Liverpool, had errors of between  $5^\circ$  and  $6^\circ$  on some points. It occurred to him that this error was caused by the length of the needle and the proximity of the correctors, and to test this he made experiments on the deviation produced on needles of different length by magnets and soft iron with the following results.

With 3-inch single needles deflected by magnets, the deviations were nearly "semicircular;" but with 6-inch needles, and still more strongly with 12-inch needles, a "sextantal" error of very considerable magnitude was introduced.

With soft iron correctors deflecting a  $7\frac{1}{2}$ -inch single needle, in addition to the "quadrantal" deviation, a considerable "octantal" error was introduced.

When the same experiments were made with an Admiralty standard compass card, constructed as usual with four parallel needles, the extremities of which are  $15^\circ$  and  $45^\circ$  on each side of the extremities of the diameter to which they are parallel, *there was no appreciable sextantal or octantal deviation*. And on investigating the subject mathematically, it appeared that *this arrangement of needles, or the simpler arrangement of two needles each  $30^\circ$  on each side of the diameter, produces a complete compensation and correction of these errors*.

The formulæ are the following:—The deviation produced in a single needle of length  $2a$  by a magnetic particle  $M$  at the same level and at a distance  $b$ , is

$$\frac{M}{b^3} \cdot \frac{1}{H} \left\{ \left( 1 + \frac{3a^2}{8b^2} \right) \sin \zeta' + \frac{15a^2}{8b^3} \sin 3\zeta' \right\},$$

giving a sextantal deviation bearing to the semicircular proportion of

$$\frac{15a^2}{8b^3} : 1 + \frac{3a^2}{8b^2}.$$

If the compass has two needles the ends of each  $a^\circ$  from the ends of a diameter, the deviation is

$$\frac{M}{b^3} \cdot \frac{1}{H} \left\{ \left( 1 + \frac{3a^2}{8b^2} \right) \sin \zeta' + \frac{15}{8} \cdot \frac{a^2 \cos 3\alpha}{b^3 \cos \alpha} \cdot \sin 3\zeta' \right\}.$$

So that if  $\alpha = 30^\circ$ , or if the two needles be each  $30^\circ$  on each side of the diameter which is parallel to them, the sextantal term disappears.

If we have four needles the ends of each pair  $\alpha^\circ$  and  $\alpha'^\circ$  from the ends of a diameter, the sextantal term has a factor,

$$\cos 3 \frac{\alpha + \alpha'}{2}.$$

Showing that if, as in the Admiralty compass, the needles of each pair are placed at equal distances on each side of the lines of  $30^\circ$ , the sextantal deviation will be reduced to zero.

A similar investigation shows that the same arrangement of needles reduces to zero the octantal error introduced by the too great proximity of the soft iron ; and further, the error introduced by the magnetism of the needle inducing magnetism in the soft iron in its vicinity.

The conclusion of the authors is that by the use of the Admiralty standard compass, or of a compass with two needles each  $30^\circ$  from the diameter parallel to them, the correcting magnets and soft iron correctors may be placed much nearer the compass than can safely be done with a single needle compass card, and the large deviations found in iron ships far more accurately corrected.

*Correction of the Quadrantal Deviation.*—It has long been known that two compasses placed as in the common double binnacle, produce in each other a *negative* quadrantal deviation. The discussion by Mr. Evans of the deviations of all the iron-built ships in the Royal Navy (Phil. Trans. 1860, p. 337), showed that the quadrantal deviation in such ships is always *positive* ; and as there is great difficulty and inconvenience in the usual mode of correcting large quadrantal deviation by soft iron, it occurred to Mr. Evans that the correction might be made by the reciprocal action of two compasses placed at the distance of 18 to 24 inches from each other, as in the common double binnacle. The precautions to be used are that the two compasses must be of equal power, and as the correction is inversely as the earth's horizontal force at the place, if great accuracy is required there ought to be the means of adjusting the distance of the two compasses, and the method will probably be found inapplicable in very high magnetic latitudes.

The deflection so produced by one compound compass on a like compass is

$$-\frac{3M}{\delta^3 H} a \cos \alpha \left\{ \left( 1 + \frac{5}{3} \frac{a^2}{\delta^2} \cos^2 \alpha \right) \sin 2 \zeta' - \frac{35}{\delta} \frac{a^2 \cos 3 \alpha}{\delta^3 \cos \alpha} \sin 4 \zeta' \right\}.$$

In this arrangement therefore an octantal error is introduced which may be avoided by the use of two Admiralty standard cards, or cards with two needles each  $30^\circ$  from the diameter which is parallel to them.



April 25, 1861.

Major-General SABINE, R.A., Treasurer and Vice-President,  
in the Chair.

The following communications were read :—

- I. "On the Distribution of Aqueous Vapour in the upper Parts of the Atmosphere." By Lieut.-Col. RICHARD STRACHEY, F.R.S. Received March 25, 1861.

The experiments of chemists having shown that any gas will flow into a space occupied by another gas, and diffuse itself there as though the space were a vacuum and the second gas not there, it was suggested by Dalton that the atmosphere might be considered to be a combination of as many distinct atmospheres as it has gaseous components, and that the actions of each of these might be treated of separately, and irrespective of the others. Meteorologists, pursuing this idea, have proposed to separate the pressure of the aqueous vapour from the whole barometric pressure of the atmosphere, and thence to infer the pressure of the permanently elastic portion, or as it has been called, the *Gaseous Pressure*, or the *Pressure of the Dry Air*.

It is my object to inquire how far the facts of the matter will support Dalton's suggestion of the possible independent existence of an atmosphere of aqueous vapour, and whether we can in truth eliminate the pressure of this vapour by subtracting the observed tension from the total barometric pressure, in the manner that has commonly been done of late years.

And first as to the hypothesis of an atmosphere of aqueous vapour pressing only upon itself. If there be such an atmosphere, the general laws of pressure of elastic fluids will apply to it, as they do to the mixed atmosphere. But in consequence of the small specific gravity of the vapour, the rate of the diminution of pressure in the upper strata of the vapour atmosphere would be much slower than in the mixed atmosphere; and irrespective of any variation in the law of the decrease of temperature, the height to which we should have to ascend in the vapour atmosphere to produce a given diminution of pressure, would be to the corresponding height in the mixed atmo-

sphere inversely as the specific gravities of the atmospheres, that is, as 1 to .625 or as 8 to 5. Thus by ascending about 19,000 feet in the atmosphere, the barometric pressure is found to be reduced one-half; and consequently it would be necessary to ascend to about  $\frac{2}{3}$  of 19,000 feet, or upwards of 30,000 feet, to produce a corresponding diminution of pressure in a vapour atmosphere, or to reduce the tension, say from 1 inch to  $\frac{1}{2}$  an inch.

Now let us compare this result with the observed facts. This is done in the annexed Table I., in which the ratio of the tensions of the vapour at heights extending to 20,000 feet with the surface tension, as actually observed, is set down in juxtaposition with the ratios that should hold good in an independent vapour atmosphere. We here see that in reality the tension is reduced to one-half of what it is at the earth's surface by an ascent of about 8000 feet, instead of 30,000 feet, as the hypothesis of the independent vapour atmosphere would require.

TABLE I.

*Comparison of Tensions of Dalton's Hypothesis with those actually observed.*

Height above the earth's surface in feet.	Barometric pressure of the entire atmosphere.	Calculated tensions in atmosphere of aqueous vapour alone, that at the earth's surface being 1.0.	Ratio of tensions to surface tension actually observed.		
			By Dr. Hooker on the Himalaya.	By Mr. Welch in four balloon ascents.	At Dodabetta & Mahabaleshwar (See Col. Sykes).
0	30.0	1.00	1.00	1.00	
2,000	28.0	.96	.82	.88	
4,000	26.1	.92	.68	.77	.67
6,000	24.3	.88	.62	.58	
8,000	22.6	.84	.52	.45	.47
10,000	21.0	.80	.42	.35	
12,000	19.5	.77	.35	.30	
14,000	18.0	.73	.29	.19	
16,000	16.6	.70	.25	.18	
18,000	15.3	.67	.20	.16	
20,000	14.1	.64	.16	.12	

The facts, as indicated by the long series of observations, of which the Table contains an abstract, are altogether in accordance with the results of my own observations; but I have thought it better to rest my conclusions on the facts observed by others. Further, the discrepancy between the observations and the hypothesis is so great, and so constant with reference to all the localities, the Himalaya, En-

gland, and the mountains of the south of India, while the observations are themselves so thoroughly consistent, that the conclusion is inevitable that the hypothesis is untenable.

A similar conclusion as to the entire incompatibility of the hypothesis of a separate vapour atmosphere with the facts, may be drawn quite independently of any observation of tensions, from a mere consideration of the known laws of the diminution of temperature as we ascend. An argument, something to this effect, will be found in Bessel's paper on Barometric Heights \*; but its form being too mathematical to be generally intelligible, I shall endeavour to place the matter in a rather more popular point of view.

Let us suppose, then, that we are at a place at the sea-level where the temperature of the air is  $80^{\circ}$ , the tension of vapour being  $\cdot 80$ , which would make the dew-point  $72^{\circ}\cdot 5$ —a case that must be of constant occurrence. If, now, we rose gradually above the earth's surface, the temperature of the air would be reduced at the known rate of about  $3^{\circ}$  for 1000 feet; while the tensions of vapour, and the corresponding dew-points, calculated upon the hypothesis of an atmosphere of vapour pressing upon itself, would be as follows:—

At	ft.	Tension	Dew-point	Air
0	..	$\cdot 80$	$72^{\circ}\cdot 5$	$80^{\circ}\cdot 0$
1000	..	$\cdot 78$	$71^{\circ}\cdot 8$	$77^{\circ}\cdot 0$
2000	..	$\cdot 77$	$71^{\circ}\cdot 2$	$74^{\circ}\cdot 0$
3000	..	$\cdot 75$	$70^{\circ}\cdot 5$	$71^{\circ}\cdot 0$
4000	..	$\cdot 74$	$69^{\circ}\cdot 9$	$68^{\circ}\cdot 0$

Hence, up to about 3000 feet, the temperature of the air would be found to be higher than the dew-point, and the supposed tensions might of course exist. But the temperature of the air, it will be seen, diminishes much more rapidly as we ascend than that of the dew-point; and the former will therefore soon fall below the latter. Thus at 4000 feet, the air being at  $68^{\circ}$ , the theory demands vapour, with a dew-point of  $69^{\circ}\cdot 9$ , which is impossible; for any vapour, in excess of that corresponding to the air temperature  $68^{\circ}$ , would be instantly precipitated. In like manner it might be shown that, under all conceivable conditions of heat or cold, and of damp or dryness at the surface of the earth, we could always ascend to a height where

\* *Astronomische Nachrichten*, Nos. 356, 357; and *Taylor's Scientific Memoirs*, vol. ii. p. 517.

the diminution of temperature would render the progression of the tensions according to the presumed law impossible. We may therefore conclude generally, that the known diminution of temperature in the atmosphere is incompatible with the existence of so large a quantity of vapour in the upper strata as the theory in question demands; and, consequently, that the tensions observed at the surface are neither dependent on, nor balanced by, the pressure of the vapour in the higher parts of the atmosphere (in the way in which the entire barometric pressure depends on the weight of the whole superincumbent column of air), for this would be insufficient to produce them. To render an independent vapour atmosphere possible would, indeed, require a fall of temperature in the air of about  $1^{\circ}$  for 1500 feet, or less than a quarter of that which really takes place.

It will also follow that, as the tension of vapour at any point exceeds the sum of all the pressures of the vapour above it, it must in part be due to the reaction of the air particles, which must therefore press upon those of vapour, contrary to the supposition with which we started. This is, in fact, equivalent to saying that the air offers a resistance to the diffusion of vapour, instead of having no effect whatever in obstructing it; and thus from an erroneous assumption, based upon experiments made on very small quantities of air in confined vessels, arises the fallacy of the theory I have been considering.

I am aware of no systematic observations relative to the actual distribution of vapour in the atmosphere, excepting those made by Dr. Joseph Hooker, and published in his *Himalayan Journals*\*. He found in his journeys in Sikim, which extended to heights of 18,000 feet and upwards, that the quantity of vapour was dependent rather on the temperature of the air than on anything else, and that it was, in fact, simply a certain proportion of the maximum quantity that can exist in accordance with the conditions of temperature at any altitude, the relative quantity being pretty nearly constant throughout the whole column. These conclusions of Dr. Hooker are altogether corroborated by my own observations. In the annexed Tables I have further illustrated this. In Table II. I have shown for a considerable range of temperature at the earth's surface, the proportion of vapour that would be found at various heights in the atmo-

\* *Himalayan Journals*, vol. ii. p. 422.

sphere, as compared to that at the surface (which is in each case assumed to be represented by 1·0), supposing the air to be everywhere saturated with moisture, and the reduction of temperature for ascent to be 3° for 1000 feet. To these calculated ratios are added those actually observed by Dr. Hooker in the Eastern Himalaya, and by Mr. Welsh in his balloon ascents\*, as already given in a preceding page.

TABLE II.  
*Proportion of Vapour at various Altitudes.*

Height in feet.	Calculated, the temperature of air at the sur- face being				Observed,	
	80°.	60°.	40°.	20°.	By Dr. Hooker, in Sikim—Therm. at sea-level being 70° to 90°.	By Mr. Welsh, in a Balloon—Therm. at surface being 50° to 70°.
0	1·00	1·00	1·00	1·00	1·00	1·00
2,000	·82	·81	·79	·77	·82	·88
4,000	·67	·65	·62	·58	·68	·77
6,000	·54	·52	·48	·44	·62	·58
8,000	·44	·41	·36	·34	·52	·45
10,000	·35	·32	·28	·26	·42	·35
12,000	·28	·25	·21	·19	·35	·30
14,000	·22	·19	·16	·14	·29	·19
16,000	·18	·15	·12	·10	·25	·18
18,000	·14	·12	·09	...	·20	·16
20,000	·11	·08	·07	...	·16	·12

In Table III. the results of Dr. Hooker's and my own observations are given in more detail.

The accordance between the calculated and observed quantities of vapour shown by these figures is so close, that we can have no hesitation in admitting that Dr. Hooker's conclusion will, in all probability, be found to afford the general solution of this problem. The relative quantity of vapour in the case of his observations having been rather greater in the higher than in the lower strata of the atmosphere, is a circumstance which cannot be held to affect the general truth of our conclusions, as will be perfectly accounted for by supposing that the diminution of temperature with height was less rapid in reality, than 3° for 1000 feet, on which my calculation is based. It is further worthy of notice, that the calculated proportion of vapour at various altitudes varies but little even with considerable change of the surface temperature, though there is a manifest ten-

\* Phil. Trans. 1853, p. 311.

TABLE III.

*Ratio of Tensions observed at various Altitudes on the Himalaya to the Tension at the Sea-level.*

Height.	Jan.		Feb.		March.		April.		May.		June.		July.		August.		Sept.		Oct.		Nov.		Dec.		Mean.		General mean by observations.	Theoretical ratio by calculation.
	A.	B.	A.	B.	A.	B.	A.	B.	A.	B.	A.	B.	A.	B.	A.	B.	A.	B.	A.	B.	A.	B.	A.	B.	A.	B.		
Sea-level	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
1 to 2,000 feet	.95	.83	.95	.83	.95	.83	.95	.83	.95	.83	.95	.83	.95	.83	.95	.83	.95	.83	.95	.83	.95	.83	.95	.83	.95	.83	.95	.83
2,000 "	.78	.69	.78	.69	.78	.69	.78	.69	.78	.69	.78	.69	.78	.69	.78	.69	.78	.69	.78	.69	.78	.69	.78	.69	.78	.69	.78	.69
4,000 "	.70	.61	.70	.61	.70	.61	.70	.61	.70	.61	.70	.61	.70	.61	.70	.61	.70	.61	.70	.61	.70	.61	.70	.61	.70	.61	.70	.61
6,000 "	.68	.59	.68	.59	.68	.59	.68	.59	.68	.59	.68	.59	.68	.59	.68	.59	.68	.59	.68	.59	.68	.59	.68	.59	.68	.59	.68	.59
7,000 "	.65	.56	.65	.56	.65	.56	.65	.56	.65	.56	.65	.56	.65	.56	.65	.56	.65	.56	.65	.56	.65	.56	.65	.56	.65	.56	.65	.56
8,000 "	.63	.54	.63	.54	.63	.54	.63	.54	.63	.54	.63	.54	.63	.54	.63	.54	.63	.54	.63	.54	.63	.54	.63	.54	.63	.54	.63	.54
9,000 "	.60	.51	.60	.51	.60	.51	.60	.51	.60	.51	.60	.51	.60	.51	.60	.51	.60	.51	.60	.51	.60	.51	.60	.51	.60	.51	.60	.51
10,000 "	.58	.49	.58	.49	.58	.49	.58	.49	.58	.49	.58	.49	.58	.49	.58	.49	.58	.49	.58	.49	.58	.49	.58	.49	.58	.49	.58	.49
11,000 "	.56	.47	.56	.47	.56	.47	.56	.47	.56	.47	.56	.47	.56	.47	.56	.47	.56	.47	.56	.47	.56	.47	.56	.47	.56	.47	.56	.47
12,000 "	.54	.45	.54	.45	.54	.45	.54	.45	.54	.45	.54	.45	.54	.45	.54	.45	.54	.45	.54	.45	.54	.45	.54	.45	.54	.45	.54	.45
13,000 "	.52	.43	.52	.43	.52	.43	.52	.43	.52	.43	.52	.43	.52	.43	.52	.43	.52	.43	.52	.43	.52	.43	.52	.43	.52	.43	.52	.43
14,000 "	.50	.41	.50	.41	.50	.41	.50	.41	.50	.41	.50	.41	.50	.41	.50	.41	.50	.41	.50	.41	.50	.41	.50	.41	.50	.41	.50	.41
15,000 "	.48	.39	.48	.39	.48	.39	.48	.39	.48	.39	.48	.39	.48	.39	.48	.39	.48	.39	.48	.39	.48	.39	.48	.39	.48	.39	.48	.39
16,000 "	.46	.37	.46	.37	.46	.37	.46	.37	.46	.37	.46	.37	.46	.37	.46	.37	.46	.37	.46	.37	.46	.37	.46	.37	.46	.37	.46	.37
17,000 "	.44	.35	.44	.35	.44	.35	.44	.35	.44	.35	.44	.35	.44	.35	.44	.35	.44	.35	.44	.35	.44	.35	.44	.35	.44	.35	.44	.35
18,000 "	.42	.33	.42	.33	.42	.33	.42	.33	.42	.33	.42	.33	.42	.33	.42	.33	.42	.33	.42	.33	.42	.33	.42	.33	.42	.33	.42	.33
19,000 "	.40	.31	.40	.31	.40	.31	.40	.31	.40	.31	.40	.31	.40	.31	.40	.31	.40	.31	.40	.31	.40	.31	.40	.31	.40	.31	.40	.31

NOTES.

A. Ratios from observations by Dr. Hooker in Sikkim, referred to Calcutta.

B. Ratios from observations by Lieut.-Col. Strachey in Kumaon, referred to Fotebgarh.

The Theoretical ratios are calculated as explained; the temperature at the surface being taken as 50°, and the decrement 3° in 1000 feet.

dency for the upper strata to contain a rather higher per-centage with a high than with a low temperature; a result likewise in accordance with fact, so far as we are able to judge from the comparison of Dr. Hooker's Indian observations with those made in England by Mr. Welsh.

The precise determination of the entire pressure of the vapour thus shown by observation to be suspended in the atmosphere is a matter of some difficulty; but an approximation may be made to it as follows:—

Let us suppose the weight of the vapour to be measured, as is often done in the case of the entire atmosphere, by the height of a column of the density observed at the surface. The height of a homogeneous atmosphere of vapour, equivalent to an independent vapour atmosphere, on Dalton's hypothesis would obviously be  $\frac{2}{3}$  of the height of the homogeneous air atmosphere, that is  $\frac{2}{3}$  of 26,250 feet, or about 42,000 feet.

But the vapour actually existing is much less than this. Taking the results of Dr. Hooker's observations, and considering the density at the surface to be unity, the mean density of the whole vapour below 20,000 feet will readily be calculated to be about  $\cdot 47$ ; so that the whole of the vapour up to this height would be equivalent to a homogeneous column of 9460 feet of density 1.0. Now it may be assumed approximately that the quantity of vapour above 20,000 feet will bear the same relation to the entire quantity, as holds good between the densities at that height and at the surface; and as we see from the Table that the density at 20,000 feet is  $\frac{1.6}{10.0}$  of what it is at the surface, we may infer that this is the proportion of the vapour above that altitude, the remainder, or  $\frac{8.4}{10.0}$ , being below it. Consequently the whole quantity of vapour, according to Dr. Hooker's observations, would be equivalent to a homogeneous column of  $\frac{10.0}{8.4} \times 9460$ , or 11,260 feet. Using the balloon observations, the height would be rather less than this, viz. 10,050 feet, so that we may infer that the actual pressure of the vapour in the atmosphere is to that represented by the tension at the surface of the earth, as 10,500 to 42,000, or as about one to four; and this ratio would also subsist between the actual pressures and observed tensions at all elevations.

The problem might otherwise be solved, by comparing the diminution of density as we ascend, according to Dalton's hypothesis,

and the observations, as shown by the series of figures in Table I. This diminution, it will be seen, takes place in all the series, approximately in a geometrical ratio, so that the density is reduced nearly in an equal proportion for each 2000 feet of ascent, namely, from 1·00 to ·96, that is by  $\frac{4}{100}$ , on Dalton's hypothesis; from 1·00 to ·84, that is by  $\frac{16}{100}$ , according to Dr. Hooker; and from 1·00 to ·82, that is by  $\frac{18}{100}$ , according to Mr. Welsh. Now it follows, from an obvious mathematical law, that the entire quantities of vapour in these different cases are inversely proportional to the constant reduction of density; so that the quantity on Dalton's hypothesis, which is that represented by the observed tension at the surface, is to the quantity according to Dr. Hooker, as sixteen to four, and to the quantity according to Mr. Welsh, as eighteen to four, a result nearly identical with the former. The subtraction of the observed tension of vapour from the total barometrical pressure, in the hope of obtaining the simple gaseous pressure, must consequently be denounced as an absurdity; and the barometrical pressure thus corrected, as it is called, has no true meaning whatever.

In conclusion, I would remark that the consideration of the small quantity of vapour that is disseminated in the upper parts of the atmosphere, shows us that inequalities of level on the earth's surface, which are insignificant when viewed in relation to the dimensions of the globe, become objects of the greatest importance in connexion with the atmosphere which surrounds it. Three-fourths of the whole mass of the air is within range of the influence of the highest mountains; one-half of the air and nearly nine-tenths of the vapour are concentrated within about 19,000 feet of the sea-level, a height which hardly exceeds the mean level of the crest of the Himalaya; while one-fourth of the air and one-half of the vapour are found below a height of 8500 feet. Thus, mountains even of moderate magnitude may produce important changes in very large masses of the atmosphere, as regards their movements, their temperature, and their hygrometric state; and especially in those strata that contain the great bulk of the watery vapour, and that have the greatest effect therefore in determining the character of climate.



## II. "On the Synthesis of Succinic and Pyrotartaric Acids."

By MAXWELL SIMPSON, Esq., M.B. Communicated by  
Dr. FRANKLAND. Received April 10, 1861.

(Abstract.)

Since my last communication to the Society\*, I have succeeded in obtaining the cyanide of ethylene in a state of purity by a slight modification of the process I have already given. A detailed account of it will be found in the paper which accompanies this abstract.

This is, I believe, the first example of a diatomic cyanide. It has the following properties in addition to those I have already enumerated:—Below the temperature of 37° Cent. it is a crystalline solid of a light-brown colour, above that temperature it is a fluid oil. Its specific gravity at 45° Cent. is 1.023. It has an acrid disagreeable taste. It is neutral to test-paper. It is decomposed by potassium, cyanide of potassium being formed. Its solution in water is not affected by nitrate of silver. Heated with nitric acid, it gives succinic acid and nitrate of ammonia. Heated with muriatic acid, it yields the same acid and muriate of ammonia. It forms an interesting compound with nitrate of silver, which was obtained in the following manner:—About three equivalents of crystallized nitrate of silver were rubbed up in a mortar with one equivalent of pure cyanide of ethylene and a considerable quantity of ether. The ether was then poured off, and the residual salt dissolved in boiling alcohol. On cooling, the alcohol became a mass of brilliant pearly plates. Submitted to analysis, these yielded results agreeing with the formula  $C_2H_4Cy_2 + 4(AgO, NO_3)$ . The crystals are soluble in water and alcohol, insoluble in ether. When heated, they melt and explode like gunpowder. They do not detonate on percussion. This compound may possibly throw some light on the constitution of the fulminates.

I have also slightly modified the process I gave in my last note for succinic acid. The modified process is very productive, and yields the acid at once in a state of purity. From 1500 grains of bromide of ethylene I obtained 480 grains of succinic acid, or nearly

\* Proceedings, vol. x. p. 574.

33 per cent. It gave on analysis 40.54 instead of 40.67 per cent. of carbon.

We are now enabled, thanks to the researches of Messrs. Perkin and Duppa and of M. Kekulé\*, to build up three highly complex organic acids (succinic, paratartaric, and malic) from a simple hydrocarbon; and, what is more important, we are enabled to do this by processes every stage of which is perfectly intelligible.

With the view of ascertaining whether or not the homologues of succinic acid could be obtained in a similar manner, I have endeavoured to prepare pyrotartaric acid from the cyanide of propylene, propylene being the radical of propylglycol.

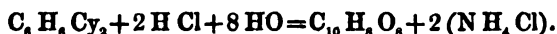
*Preparation of Cyanide of Propylene.*—A mixture of one equivalent of bromide of propylene and two of cyanide of potassium, together with a considerable quantity of alcohol, was exposed to the temperature of a water-bath for about sixteen hours. The alcohol was then filtered and distilled. A liquid residue was thus obtained, which was dissolved in ether. The body left on evaporating the ethereal solution was then submitted to distillation. Almost the entire liquid passed over between 265° and 290° Cent. The fraction distilling between 277° and 290° Cent. was collected apart and analysed. It gave 62.0 instead of 63.8 per cent. of carbon. This body cannot be obtained purer by distillation under atmospheric pressure, as it suffers partial decomposition during the process.

The properties of this cyanide very much resemble those of the preceding. It differs, however, in its physical state, which is that of a liquid at the ordinary temperature of the air. It is soluble in water, alcohol, and ether. It has an acrid taste. It is neutral to test-paper. It is decomposed by potassium, cyanide of potassium being formed. Its solution in water does not precipitate nitrate of silver. Heated with potash, it is resolved into an acid and ammonia.

*Formation of Pyrotartaric Acid.*—A mixture of one volume of cyanide of propylene and about  $1\frac{1}{2}$  volume of strong muriatic acid was exposed in a sealed tube to the temperature of a water-bath for a few hours. On cooling, the contents of the tube became a mass of crystals. These were dried and dissolved in absolute alcohol. The residue obtained on evaporating the alcoholic solu-

\* Quarterly Journal of the Chemical Society, July 1860; and Bulletin de la Société Chimique de Paris du Août, 1860, p. 208.

tion was then twice crystallized from water, and finally digested with ether. The body left on distilling off the ether is the acid in question. The numbers obtained on analysis agree very well with the formula of pyrotartaric acid; I got 44.6 instead of 45.4 per cent. of carbon. It had also all the properties ascribed to this acid by Pelouze and Arppe. The crystals were colourless, and very soluble in water, alcohol, and ether. It had an agreeable acid taste. It became semi-fluid at 100° Cent., and melted completely a few degrees above that temperature. Long-continued ebullition in a glass tube converted it into an oil, which was insoluble in cold water, and no longer affected litmus-paper, but which gradually dissolved in hot water, recovering at the same time its acid reaction. The following equation will explain the reaction which gives birth to this acid :



It is highly probable that there exists a series of isomeric acids running parallel to these, which may be obtained by similar processes from the diatomic radicals contained in the aldehydes. Thus from cyanide of ethylidene  $(C_4 H_4 Cy_2)$  we may hope to get an isomer of succinic acid.

I propose to continue my researches in this direction, and to extend them to the cyanides of the triatomic radicals.

III. "On the Elimination of Urea and Urinary Water, in relation to Period of the Day, Season, Exertion, Food, and other influences acting on the Cycle of the Year." By EDWARD SMITH, M.D., F.R.S. Received April 15, 1861.

The reading of this paper was not concluded.

*May 2, 1861.*

Major-General SABINE, R.A., Treasurer and Vice-President,  
in the Chair.

In accordance with the Statutes, the names of the Candidates recommended by the Council for election into the Society, were read from the Chair, viz.—

Charles Spence Bate, Esq.  
Heinrich Debus, Esq.  
Campbell De Morgan, Esq.  
Thomas A. Hirst, Esq.  
A. Matthiessen, Esq.  
J. Clerk Maxwell, M.A.  
Ferdinand Müller, M.D.  
William Newmarch, Esq.

Edmund Alexander Parkes, M.D.  
William Pole, Esq.  
Philip Lutley Sclater, M.A.  
Charles Frederick Alexander  
Shadwell, Capt. R.N.  
Henry J. Stephen Smith, M.A.  
William Stokes, M.D.  
George Johnstone Stoney, M.A.

The following communications were read :—

- I. "On Internal Radiation in Uniaxal Crystals." By BALFOUR STEWART, Esq., A.M. Communicated by J. P. GASSIOT, Esq. Received April 11, 1861.

(Abstract.)

The well-known theory of exchanges, which was proposed by the late Prof. Prevost of Geneva, is built upon the fact that a substance placed anywhere within an enclosure of a constant temperature will ultimately attain the temperature of the enclosure.

In his theory M. Prevost supposes that a constant, mutual, and equal interchange of radiant heat takes place between the body and the enclosure which surrounds it, so that, receiving back precisely that heat which it gives away, the former is enabled to remain at a constant temperature.

With respect to this radiation, which is thus supposed to be constantly taking place between substances at the same temperature, it had until lately been conceived of as proceeding mainly, if not entirely, from the surface of bodies—a very thin film or plate of any

substance being supposed to furnish the maximum amount of radiation which that substance was capable of affording.

It lately occurred to the author of this paper, reasoning from the theory of exchanges, that mere surface radiation is not sufficient to account for the equilibrium of temperature which exists between a body and the enclosure which surrounds it.

These theoretical conclusions have been amply verified by experiment, and the subject has been discussed in a paper published in the 'Transactions of the Royal Society of Edinburgh' for the year 1858. As the chain of reasoning by which this fact is deduced theoretically from the law of exchanges, and the experimental evidence upon which it rests, are both of a very simple nature, it has been thought well to restate them here before proceeding further in this investigation.

Let us imagine to ourselves an enclosure of lamp-black kept at a constant temperature, and containing two pieces of polished rock-salt similar to one another, except that the thickness of the one is greater than that of the other.

Now it is evident that since the thick piece absorbs more of the heat which falls upon it from the sides of the enclosure than the thin piece, it must likewise radiate more in order that it may always remain at the same temperature. Here then we have the fact of internal radiation in the case of rock-salt deduced as a theoretical consequence of the law of exchanges; experimentally it is found that a thick piece of rock-salt radiates very considerably more than a thin piece.

The fact of internal radiation being conceded, it is easy to see that the amount of heat which a particle radiates must be independent of its distance from the surface. For besides that this is the simplest hypothesis, the absorption, and consequently the radiation of two similar plates of rock-salt placed with their surfaces together, ought to be the same as from a single plate of double the thickness; and experiment shows that this is the case.

It being therefore supposed that the internal radiation of a particle is independent of its distance from the surface, let us imagine a row of particles A, B, C, D in the midst of a substance of constant temperature which extends indefinitely on all sides of them. There will be a certain stream of radiant heat constantly flowing past any such particle A to go in the direction AB.

Now, since the radiation is supposed to be the same for the different particles A, B, C, D, it follows that the absorption of the stream of heat by these particles must also be the same for each; and in order that this may be the case, it is necessary that the stream which impinges on one particle be the same in quantity and in quality as that which impinges upon another. This consideration leads us to a method of viewing internal radiation, which is wholly independent of the diathermanous or athermanous character of the body. For whatever be the absorption of a particle for any description of heat, its radiation must necessarily be precisely the same in order that the stream of heat in passing the particle may be just as much recruited by its radiation as it is reduced by its absorption; in other words, we may regard the substance through which the heat passes as perfectly diathermanous.

We gain another advantage by this method of viewing the subject: for, in the law which is expressed by saying that the absorption of a particle is equal to its radiation, and that for every description of heat, the word *description* is used to define and separate those rays of heat which are absorbed in different proportions by the same substance. Therefore in any problem connected with this subject we may suppose that a separate equilibrium holds for every such ray.

Now it is well known that rays of different wave-lengths are absorbed in different proportions by the same substance. We are therefore entitled to suppose that a separate equilibrium holds for each wave-length. The advantage of this is obvious in problems which admit of the application of optical principles. But we may go even further. For we know that in tourmaline, and in some other crystals cut parallel to the optic axis, the ordinary ray is more absorbed than the extraordinary; and the experiments of Prof. Kirchhoff and the author have shown that in tourmaline the ordinary ray is also radiated in excess. It thus appears that, in the case of crystals, we have not only a separate equilibrium for each wave-length, but for each of the two rays into which the incident ray is divided.

The following method of comparing together two streams of radiant heat has been adopted:—Consider a square unit of surface to be placed in the midst of a solid of indefinite thickness on all sides, and find the amount of radiant heat which passes across this square unit of surface in unit of time in directions very nearly perpendicular to

the surface, and comprehending an exceedingly small solid angle  $\delta\phi$ . Call this heat  $R\delta\phi$ , then  $R$  may be viewed as the intensity of the radiation in this direction.

Let us now suppose that we have a uniaxal crystal of indefinite thickness bounded by a plane surface, and that parallel to this surface, and separated from it by a vacuum, we have a surface of lamp-black, the whole being kept at a constant temperature.

Let us take a square unit of this surface, and consider the heat from the lamp-black which falls upon it through an exceedingly small solid angle in a direction not necessarily perpendicular to the surface. Part of this heat will be refracted into the interior of the crystal in two rays, the ordinary and the extraordinary. There will be thus two separate bundles of refracted rays, the solid angle comprised by the individual rays of the one being different from that comprised by the rays of the other; the inclination to the surface also being different for each bundle.

Now, on the principle of a separate equilibrium for each ray, these entering bundles of rays must respectively equal the rays of the same kind which emerge from the crystal in the same directions.

Hence if we know the radiation of lamp-black, and the direction in which the rays under consideration strike the surface of the crystal, as also the angle which the latter makes with the optic axis, it is conceivable that, by means of optical principles, joined to the fact of the equality between the entering and emerging bundles of rays, we may be enabled ultimately to ascertain the internal radiation through the crystal in different directions.

A little consideration, however, will show that this method of procedure presupposes a certain mutual adaptation to exist between the optical principles employed and the theory of exchanges. For it is evident that the expression for the internal radiation in any direction may be obtained by operating upon terminal surfaces bearing every possible inclination to the optic axis.

But the internal radiation, if the law of exchanges be true, is clearly independent of the position of this surface, which is indeed merely employed as an expedient. This is equivalent to saying that the constants which define the position of the bounding surface must ultimately disappear from the expression for the internal radiation.

The author then endeavours to show that such an adaptation does

really exist, and that the expression for the internal radiation is independent of the position of the surface.

For the extraordinary ray, the internal radiation is found to be

$$R_e = \frac{Rr^4}{2m^4n^2}$$

where  $R$  is the radiation from lamp-black ;

and for the ordinary,  $R_o = \frac{R}{2n^2}$  ;

where  $n$  denotes the axial and  $m$  the equatorial radius of the ellipsoid into which the extraordinary ray will have spread in the crystal in the same time that *in vacuo* it would have spread into a sphere whose radius = unity ; and lastly,  $r$  denotes the radius of this ellipsoid in the direction in which the internal radiation is measured.

The author concludes by remarking that the fundamental law, which is intimately connected with the theory of exchanges, and which renders an equilibrium of temperature possible in the case under consideration, seems to be the law of the equality between action and reaction in the impact of elastic bodies.

He also considers that the law which is expressed by saying "That the absorption of a particle is equal to its radiation, and that for every description of heat," expresses another law of action and reaction which holds when the motion which constitutes radiant heat is not conveyed from particle to particle without loss, or when the bodies under consideration are not perfectly elastic.

These two laws of action and reaction are viewed as supplementing each other, so as to render that equilibrium of temperature which is demanded by the theory of exchanges possible under all circumstances.

II. "On Fermat's Theorem of the Polygonal Numbers." By the Right Hon. Sir FREDERICK POLLOCK, Lord Chief Baron. Received July 11, 1860. Revised by the Author April 25, 1861.

(For Abstract, see Vol. X. p. 571.)



III. "Note on Professor Faraday's Recent Experiments on 'Regelation.'" By Professor JAMES THOMSON, Queen's College, Belfast. Communicated by Professor WILLIAM THOMSON. Received April 25, 1861.

Some time ago\*, Principal James D. Forbes showed that two slabs of ice, having each a face ground tolerably flat, and being both suspended in an atmosphere a little above the freezing-point upon a horizontal rod of glass passed through two holes in the plates of ice, so that the plates might hang vertically and in contact with one another, would unite gradually so as to adhere strongly together. This interesting experiment Principal Forbes adduced as being in opposition to the theory offered by me† of the plasticity of ice, and of the tendency of pieces of thawing ice to unite when placed in contact. He thought it showed that pressure was not essential to the union of the two pieces of ice. I pointed out, in reply‡, that the film of water between the two slabs, being held up against gravity by the capillary tension or contractile force of its free upper surface, and being distended besides against the atmospheric pressure, by the contractile force of its free surface round its whole perimeter—except for a very small space at bottom, from which water trickles away, or is on the point of trickling away,—exists under a pressure which, though increasing from above downwards, is everywhere, except at that little space near the bottom, less than atmospheric pressure :—that hence the two slabs are urged against one another by the excess of the external atmospheric pressure above the internal water pressure, and are thus pressed against one another by a force quite notable in amount ;—that, further, the film of water existing as it does, under less than atmospheric pressure, has its freezing-point raised in virtue of the reduced pressure ; and would therefore freeze even at the temperature of the surrounding ice, which I took to be the freezing-point for atmospheric pressure ; and would still more strongly

\* "On some Properties of Ice near its Melting-Point." By Prof. Forbes, Proceedings of the Royal Society of Edinburgh, April 1858.

† Proceedings of Royal Society, May 1857, and British Association Reports, 1857.

‡ Proceedings of Royal Society, Nov. 24, 1859, vol. x. p. 159.

be impelled to freeze by the joint action of this condition with the cold given out in the melting by pressure of the ice at the points of contact where the two slabs are urged against one another.

To this explanation of Principal Forbes's experiment I still adhere as mainly correct, though admitting of some further development and slight modification in reference to a point to which I shall have to make further allusion in what follows, and which seems to me to be as yet rather obscure:—the influence, namely, of the tension in the ice due to its own weight, which makes it *not* be subject internally to simply atmospheric pressure:—and though I shall also, in what follows, point out some additional conditions, almost necessarily present in the experiment, which, under my general view of the plasticity of ice, would act in conjunction with those already adduced, and would increase the rapidity of the union.

Professor Faraday, holding it in view to remove the ground on which my explanation of Principal Forbes's experiment was founded, has contrived and carried out a set of new and very beautiful experiments from which the capillary action referred to has been completely eliminated, and he has still found the union of the ice to occur, and to increase with time, and has met with a curious additional phenomenon of "*flexible adhesion*"\*. In these experiments, when two pieces of ice, rounded so as to be convex at their points where mutual contact is to be allowed, are placed in water, and are either anchored so as to be wholly under water, or are placed floating when so formed that they can touch one another only under water, and that, at the water surface, there shall be a wide space between them so that there shall be no capillary action drawing them together, he showed that the pieces of ice, in either of these cases, if brought gently into contact, will adhere together; unless indeed the movement bringing them into contact be so directed as to introduce forces capable of tearing them apart again by obliquity of action, by agitation of the water, or by other disturbances. He showed also that, if when the two pieces of ice have become attached at their point of contact, a slight force, such as may be given by one or two feathers, be applied, tending to separate them, at one side of their point of contact, they will roll round one another with a seemingly flexible adhesion; or that, if the point of a floating wedge-shaped

\* Proceedings of Royal Society, April 26, 1860, vol. x. p. 440.

piece of ice is brought under water against the side of another floating piece, it will stick to that piece like a leech. He showed that if the pieces be allowed to remain for a few moments in contact, their adhesion will become rigid, so that on a force being applied sufficient to break through the joint, the rupture will occur with a crackling noise, though the pieces may still continue to hold together, rolling on one another with the flexible adhesion. He made some other experiments nearly the same as these, but in which he showed the flexible and rigid adhesion to occur while there is constantly a decided tensile force applied externally tending to pull the pieces asunder instead of any external force tending to press them together. He thinks that the phenomena of flexible and rigid adhesion "under tension" go towards showing that pressure is not necessary to "regelation." He then gives his own idea of the flexible and rigid adhesion in the following words:—"Two convex surfaces of ice come together; the particles of water nearest to the place of contact, and therefore within the efficient sphere of action of those particles of ice which are on both sides of them, solidify; if the condition of things be left for a moment, that the heat evolved by the solidification may be conducted away and dispersed, more particles will solidify, and ultimately enough to form a fixed and rigid junction, which will remain until a force sufficiently great to break through it is applied. But if the direction of the force resorted to can be relieved by any hinge-like motion at the point of contact, then I think that the union is broken up amongst the particles on the opening side of the angle, whilst the particles on the closing side come within the effectual regelation distance; regelation ensues there and the adhesion is maintained, though in an apparently flexible state. The flexibility appears to me to be due to a series of ruptures on one side of the centre of contact, and of adhesion on the other,—the regelation, which is dependent on the vicinity of the ice surfaces, being transferred as the place of efficient vicinity is changed. That the substance we are considering is as brittle as ice, does not make any difficulty to me in respect of the flexible adhesion; for if we suppose that the point of contact exists only at one particle, still the angular motion at that point must bring a second particle into contact (to suffer regelation) before separation could occur at the first; or if, as seems proved by the supervention of the rigid adhesion upon the

flexible state, many particles are concerned at once, it is not possible that all these should be broken through by a force applied on one side of the place of adhesion, before particles on the opposite side should have the opportunity of regelation, and so of continuing the adhesion."

The interpretation thus put by Prof. Faraday on his experiments is not convincing to me; but, on the contrary, I think the experiments are in perfect accordance with my own theory, and tend to its confirmation. My view of the phenomena of these experiments is as follows:—The first contact of the two pieces of ice cannot occur without impact and consequent pressure; and, small as the total force may be, its intensity must be great, as the surface of contact must be little more than a geometrical point. This pressure produces union by the process of melting and regelation described by me in previous papers. On the application of the forces from the two feathers, at one side of the point of contact, tending to cause separation, the isthmus of ice formed by the union of the two pieces comes to act as a tie or fulcrum subject to tensile force; and consequently a corresponding pressure will occur at the side of the isthmus, far from the feathers; and that pressure will effect the union of the ice at the side where it occurs. The tensile force, it may readily be supposed, tends to preserve the isthmus, internally at least, in the state of ice, whatever may be its influence on the external molecules of the isthmus, and to solidify such water as, having occupied pores in the interior during previous compression, may now, by the linear tension or pull, be reduced in cubical pressure or hydrostatic pressure, because the melting-point of wet ice is raised by diminution of pressure of the water in contact with it\*. The pull applied to the isthmus

\* How the *surface* of a bar of ice immersed in cold water, as distinguished from the *interior* of the bar, may in respect to tendency either to melt away, or to solidify to itself additional ice from the water, be influenced by the application of linear tension to the bar, I am not quite prepared to say positively. The application of tension, whether linear, superficial, or cubical (that is, whether simply in one direction, or in two directions crossing one another, or in three directions crossing one another), to a piece of ice immersed in water at any given pressure, atmospheric for instance, is very distinct from the application of what might be called cubical tension, that is, diminution of hydrostatic pressure, to the surrounding water. In the former case the pressure of the water at the external surface of the ice will not be reduced by the application of the tension to the ice; though that of the water in the internal pores may, or

thus appears to put it out of the condition in which my theory has clearly indicated a cause of plasticity, and I presume makes it cease, or almost entirely cease, to be plastic. I believe no plastic yielding of ice to tension has been discovered by observation in any case, and I think there are theoretical reasons why ice should be expected to be very brittle in respect to tensile forces. The isthmus then being supposed devoid of plasticity at its extended side, ultimately breaks at that side, when the opening motion caused by the feathers has arrived at a sufficient amount to cause fracture, and the ice newly formed on the compressed side comes now to act as a tie instead of the part which has undergone disruption, and holds together the two pieces of ice, or serves as a fulcrum under tension to communicate a compressive force to the points of the two pieces of ice immediately beyond it; and so the rolling action with a constant union at the point of contact goes forward. It is to be observed that the leverage

probably in many of them must, be so; but in the second case, the diminution of cubical pressure in the external water effects the same diminution of pressure in the ice, and also in the water occupying pores in the ice. The theory and quantitative calculation which I originally gave (*Transactions Roy. Soc. Edin.* vol. xiv. part 5. 1849, and *Cambridge and Dublin Math. Journ.* Nov. 1850) of the effect of increase of pressure in lowering the freezing-point of water, and of course also of the effect of diminution of pressure in raising it, applied solely to effects of pressure communicated to the ice *through the water*, and therefore equal in all directions, and equally occurring in the ice and the water; but when changes of pressure in one or more directions are applied to the ice as distinguished from the water, the theory does not apply in any precise way to determine the conditions of the melting of the ice, or of its growth by the freezing of the adjacent water to its surface. There seems to me to be yet a field open for much additional theoretical and experimental investigation in this respect; but so far as I have applied the principle of the lowering of the freezing-point of water by pressure in developing or sketching out a theory of the plasticity of ice, I think I have done so correctly. I perceived that the application of pressures tending to change the form of the ice must necessarily produce volume-compression in some parts of the mass, accompanied by the occurrence of increased fluid pressure in the pores which might already exist in those parts, or which would arise in them as a consequence of the pressure; and this I thought was a sufficient basis on which to rest the theory, even without precise knowledge of all the varying influences on the melting or freezing of the ice or water, of all the possible varieties of pressures or stresses that could be applied to the ice, and of fluid pressure that could occur in the water contemporaneously with those stresses in the ice. Some additional developments of this part of the subject, which have occurred to me, may, I hope, form the subject of a future paper.

of the forces applied by the feathers is so great, compared with the distance from the fulcrum or tensile part of the isthmus, to the compressed part in process of formation at the other side, as that the compression may usually be considered almost equal to the tension ; and the tension in the extended part cannot be of small intensity, being sufficient to break that side of the isthmus. In the experiments which gave flexible adhesion *seemingly* under tension, it is not to be admitted that tension was really the condition under which the ice existed at the places where the union was occurring. To apply a simple disruptive force to the whole isthmus of ice, it would be necessary to take very special precautions in order to arrange that the line of application of the disruptive forces should pass through the point of contact of the two pieces. If that were done, and the forces were gradually increased till the cohesive strength of the isthmus were overcome, it is clear that the two pieces of ice would separate altogether, and there would be no flexible adhesion ; but the flexible adhesion, when it occurs, is essentially dependent on the existence of an intense pressure at the side of the isthmus remote from the line of the externally applied disruptive forces, or of the single force applied in some of the experiments to one only of the pieces, and resisted by the inertia of the other.

It is further to be observed that tremors and slight agitations to which the two pieces of ice united at their point of contact, may be subject, arising from undulations imparted to the water in which the ice is immersed, by manipulation of the experimenter,—from the tread of people on adjacent floors,—from the passage of vehicles on neighbouring streets,—from convective movements of the water,—and from other causes,—will be sources of power or energy operative in bringing about an increase of adhesion with time ; that is to say, in changing gradually the flexible into the rigid adhesion.

It will now of course be obvious that the conditions involved in the explanation just offered of Prof. Faraday's experiments must also usually be present in the experiment of Principal Forbes. Their incidental occurrence, however, as additional causes increasing the rapidity of the union of the two slabs of ice, does not overthrow the particular explanation of Principal Forbes's experiment which I had offered as a perfect answer to the objection raised by that experiment against my general view of the plasticity of ice ; and as indicating

clearly and certainly the occurrence of all the conditions required for the union of the two pieces of ice under my theory. The contingent occurrence of the additional conditions now specially brought forward, was indeed from the first somewhat familiar to my mind, but was left out of the explanation as being unessential and not perhaps quite so clearly apparent. Their occurrence has, however, now become essential to the explanation of Prof. Faraday's new experiments:—and by it I consider these are shown not to militate against my general theory of the plasticity of ice, but to corroborate it strongly, and to confirm its application to the various observed cases of the union of two pieces of moist ice when placed in contact.

May 16, 1861.

Major-General SABINE, R.A., Treasurer and Vice-President,  
in the Chair.

Sir Andrew Scott Waugh was admitted into the Society.

The CROONIAN LECTURE was then delivered by C. E. BROWN-SÉQUARD, M.D., F.R.S., "On the Relations between Muscular Irritability, Cadaveric Rigidity, and Putrefaction."

The object of this lecture is chiefly to try to establish the following law:—*The greater the degree of muscular irritability at the time of death, the later the cadaveric rigidity sets in and the longer it lasts, and the later also putrefaction appears and the slower it progresses.*

Many physiologists have already shown that in certain cases cadaveric rigidity appears late and lasts long when muscular irritability is powerful at the time of death; but the great generality of this result and its prevalence as a law, had not been established, nor had the general relation of these conditions of the muscular system to putrefaction, as stated in the above law, been enunciated before my first researches on this subject\*.

\* In 1849 and 1850 I published a summary of some experiments the object of which was to prove the existence of the above law. In a paper read at the Académie des Sciences of Paris, and in 1857, I related some new experiments

The law I wish to establish is borne out by a great many facts, observed in the lower animals and in man, showing that every cause of increase in the degree of muscular irritability a short time before death, may be considered as a cause of delay of the setting in of cadaveric rigidity and of the persistence of that *post-mortem* state of muscles, and also a cause of delay of the time at which putrefaction manifests itself in muscles, and an evident slowness of the process of putrefaction; while, on the contrary, every cause of decrease in the degree of muscular irritability some time before death, produces the opposite effect on cadaveric rigidity and putrefaction.

I will examine successively the relations existing between muscular irritability, *post-mortem* rigidity and putrefaction in the various following cases:—1st, in paralysed muscles; 2nd, in muscles the temperature of which is diminished before death; 3rd, in animals or men killed by lightning or by galvanism; 4th, in over-driven cattle, in cocks after a fight, in men after over-exertion, and in animals hunted to death; 5th, in men dying in a good state of health or after prolonged disease; 6th, in men who have died of cholera, of tetanus, or after other convulsive diseases; 7th, in men and animals killed by poison.

1st. *Irritability, cadaveric rigidity and putrefaction in paralysed muscles.*—I have shown long ago† that during a certain period after the beginning of a paralysis, the paralysed muscles are endowed with more irritability than healthy muscles. I have ascertained this fact in various ways; but the most decisive method consists in comparing the duration of irritability in a paralysed muscle and in the corresponding healthy one of the opposite side, while they are both submitted to the same excitation. I have often found in experimenting in that way, that the paralysed muscle remained irritable twice, three times, or even four times as long as the healthy one.

The following experiments, taken out of many of the same kind, while they show that muscular irritability is increased in consequence

having the same bearing. (See *Comptes Rendus de la Soc. de Biologie*, vol. i. 1849, pp. 39, 138, 154 and 178, and vol. ii. 1850, p. 194; also *Comptes Rendus de l'Acad. des Sciences*, vol. xlv. 5th Oct. 1857.)

† *Experimental Researches applied to Physiology and Pathology*. New York, 1853, pp. 68–73.



of paralysis, show also that in such conditions cadaveric rigidity sets in late and lasts long, and that putrefaction also appears late and progresses slowly.

(1) In the muscles of the leg of a dog, paralysed by section of the roots of the sciatic nerve, on one side, in the spinal canal, there was an excessive irritability at the time of death (by asphyxia), five hours after the section of the nerve-roots. In the paralysed muscles irritability lasted ten hours; cadaveric rigidity appeared nearly at the time of the cessation of irritability\*, and lasted nearly thirteen days†. Putrefaction was evident only on the beginning of the fifteenth day, and it was not very much advanced on the seventeenth day. In the healthy leg muscular irritability lasted but four hours, and was, as usual, at once followed by some degree of cadaveric rigidity. During nearly five days this rigidity lasted, and putrefaction was evident as soon as the sixth day, and much advanced on the seventh, at a time when the paralysed limb was still perfectly rigid. The temperature of the room during the fortnight the experiment lasted ranged between 6° and 11° Cent. (42°·8 to 51°·8 Fahr.) The following shows at a glance the differences between the two legs:—

	Durat. of irrit.	Durat. of cad. rigid.	Advanced putref.
Paralysed muscles . .	10 hours.	13 days.	17th day.
Healthy muscles . . .	4 hours.	5 days.	7th day.

(2) I have found that muscular irritability is very much increased in the paralysed limb of an animal on which a lateral half of the spinal cord has been divided transversely in the dorsal region. In this case, as well as after paralysis from section of a nerve, cadaveric rigidity sets in later and lasts longer, and putrefaction appears later, and proceeds slower in the paralysed hind leg (on the side of the section of the cord) than in the other.

(3) A month after the section of a nerve we have another kind of verification of the law: irritability is then diminished in the paralysed muscles; and we find that cadaveric rigidity sets in sooner and

\* Usually some muscles or some parts of a muscle are already rigid while irritability still remains in other parts. This was the case in a marked manner in the above experiment, especially in the paralysed leg.

† In dogs, cadaveric rigidity lasts longer than in most other animals. I have seen it lasting twenty-six days in a healthy dog killed by asphyxia in midwinter in Paris.

lasts a less time ; and that putrefaction appears sooner and proceeds quicker in them than in the healthy muscles.

2nd. *Influence of a diminution of temperature of muscles before death upon their irritability, cadaveric rigidity, and putrefaction.*—I found many years ago that all the vital properties of the nervous centres, the nerves and the muscles, in adult as well as in young warm-blooded animals, may be much increased in consequence of a diminution of temperature of these organs. The following experiments show that when muscular irritability is increased by that cause, the increase has the same influence upon cadaveric rigidity and putrefaction as when it is caused by paralysis.

In two kittens three days old, and of the same litter, I found after death (by asphyxia) the following differences : one of them had the temperature of 37° Cent. (98°·6 Fahr.) in the rectum at the time of death ; its muscular irritability in the posterior limbs gave way to cadaveric rigidity 3½ hours after the last effort at breathing ; this rigidity lasted nearly three days ; putrefaction began to be evident on the fourth day after death, and was much advanced the next day ; while in the other, the temperature of which was only 25° Cent. (77° Fahr.) at the time of death, muscular irritability lasted more than nine hours after the last breathing ; cadaveric rigidity began during the tenth hour and lasted nine days ; and putrefaction, which began on the tenth day, was not much advanced until two days later.

In many other instances of newly-born cats, dogs, rabbits, and birds (especially ravens, sparrow-hawks, jays, and magpies), I have observed similar differences as those observed in the preceding experiment, when I noticed the duration of irritability after death and its relations to cadaveric rigidity and putrefaction, according to the degree of animal heat at the time of death. As a general rule, when there was a difference of 8° or 10° Cent. (14° to 18° Fahr.) in the temperature of two animals of the same age and the same species, muscular irritability and cadaveric rigidity lasted twice or three times longer in the cooler animal than in the other, and putrefaction in the former was much less rapid.

I have observed the same differences also in adult birds and mammals\*. It is thus clearly shown by experiment that when the

\* For the means of diminishing the temperature of warm-blooded animals, see my *Researches on Asphyxia* (*Journal de Physiol.* Jan. 1859).

temperature of muscles is diminished before death, their irritability lasts long after death, cadaveric rigidity sets in late and lasts long, and putrefaction appears late and proceeds slowly.

3rd. *Influence of death by lightning and galvanism on cadaveric rigidity and putrefaction.*—It is well known that J. Hunter thought that cadaveric rigidity cannot take place after death by lightning; but many facts have been recently published, showing that in animals and men killed by lightning, cadaveric rigidity has existed, and there can indeed be no doubt of its occurrence in such cases. At the same time there is reason to believe that in some instances of that mode of death cadaveric rigidity has either not set in, or has been of so short a duration, that it has not been possible to ascertain its existence. It appears difficult at first sight to reconcile facts which seem to be so much in opposition one with the other; but it will be seen that the contradiction is apparent only. Death by lightning may be the result of,—1st, a syncope by fright, or in consequence of a direct or reflex influence of lightning on the par vagum; 2ndly, hæmorrhage in or around the brain or in the lungs, the pericardium, &c.; 3rdly, concussion or some other alteration of the brain. When death by lightning is due to these causes, cadaveric rigidity may, of course, appear after death, and even last long, much in the same way indeed as when death results from these causes in persons who have not been struck by lightning. But lightning may kill in another way: it may destroy life as galvanism does, by producing such a violent convulsion of every muscle in the body that muscular irritability ceases almost at once; and the ensuing rigidity may then be of so short duration as to escape notice.

The following experiments show the powerful influence of galvanism on muscular irritability, cadaveric rigidity, and putrefaction. We will at first give the results of the galvanization of one limb:—

#### *I. General Result of Experiments on adult Rabbits.*

	Galvanized limb.	Limb not galvanized.
Durat. of irritability . . . .	7 to 20 minutes	120 to 400 minutes.
Durat. of cad. rigidity . .	2 to 8 hours	1 to 8 days.
Putref. much advanced . .	within a day	only after several days.

## II. *General Result of Experiments on adult Dogs.*

	Galvanized limb.	Limb not galvanized.
Duration of irritability	12 to 25 minutes	140 to 550 minutes.
Duration of cad. rigid.	3 to 16 hours	2 to 21 days.
Putref. much advanced .	within 30 hours	only after several days.

In experimenting on the whole body of an animal I have obtained the most striking results. I will only mention here the following experiments. Five vigorous male adult guinea-pigs were asphyxiated by the application of a ligature round the trachea, and four of them were galvanized immediately,—the first with a very powerful electromagnetic machine, the second with a weaker one, the third with a still weaker one, and the fourth by only a slight galvanic current; the fifth animal was not galvanized. In the four galvanized animals the conductors were applied one to the mouth, the other to the anus.

	Durat. of irrit.	Rigid. completed.	Durat. of rigid.
1st ....	4 mins.	7 mins.	15 mins.
2nd ....	40 mins.	60 mins.	nearly 1,200 mins. (20 hrs.)
3rd ....	90 mins.	120 mins.	nearly 3,600 mins. (3 days)
4th ....	330 mins.	420 mins.	nearly 8,600 mins. (6 days)
5th ....	500 mins.	600 mins.	nearly 11,500 mins. (8 days)

As regards putrefaction, it already appeared in the first animal during the first hour after death, and was much advanced the next day; its progress was slower in the four others, and successively slower in each as compared with the preceding one in the series. Considering that the power of lightning may be vastly greater than that of our galvanic machines, we can easily understand that when lightning strikes a man or an animal in the proper place, it must produce a much greater effect than galvanism, reducing the duration of irritability to a fraction of a second, and that of cadaveric rigidity in a corresponding degree, so that no trace of it remains a few minutes after death, and in like manner hastening the access of the final process of putrefaction.

The results of the application of galvanism either to separate limbs or to the whole body of animals, appear thus to show that when lightning kills without leaving any evident alteration adequate to cause death, without the subsequent appearance of cadaveric rigidity, but with early and rapid putrefaction,—the mode of action of lightning

is similar to that of galvanism, and consists in a sudden and complete exhaustion of the nervous and muscular tissues by over-action.

I have ascertained that a galvanic current passing through a rigid muscle produces no change in the duration of cadaveric rigidity, and does not in any way hasten putrefaction. It seems very probable from this negative fact that the influence of galvanism on still irritable muscles is due to the contractions then produced, and not to some direct chemical action on the muscular tissue and on the fluids which surround the muscular fibres. This probability becomes almost a certainty when we know that any cause which produces muscular contraction acts in the same way as galvanism, as we have already seen, and as will be further shown in the course of this lecture.

4th. *Influence of prolonged muscular exercise on cadaveric rigidity and putrefaction.*—It is well known that putrefaction appears very quickly in over-driven cattle and in animals hunted to death. I had the opportunity once, in September 1851, near Dinan, in France, of observing the period of setting in of cadaveric rigidity in two sheep that had been over-driven to reach a fair. They were killed by the section of the carotid arteries. In less than five minutes after death rigidity was evident in both of them, and putrefaction was manifest before the end of the day (in less than eight hours after death).

As cadaveric rigidity lasts only a short time in cocks killed during or shortly after a fight, and also in animals hunted to death, J. Hunter was led to think that it does not come on after death in such circumstances. Mr. Gulliver has shown this opinion to be erroneous. He states that fighting-cocks, as well as hares, stags, and other animals of the chase, become rigid at once after death in the circumstances referred to. It is well known that putrefaction occurs very quickly in animals hunted to death, so that every thing we know concerning over-driven cattle, cocks after a fight, or animals hunted to death, is in perfect harmony with the law that the exercise of muscular contraction diminishes muscular irritability, and that diminished irritability is followed by early cadaveric rigidity, which, in its turn, promptly gives way to a putrefaction which progresses rapidly.

A great many facts seem to show that over-exertion acts in the same way in man as in the lower animals. The accounts given of soldiers slain on the battle-field, whose body and limbs retained the

position they were in at the moment the individuals met their death, are doubtless to be explained on the supposition that, in consequence of previous excessive exertion, rigidity came on at once. In a case of a man who kept his arms extended for a long while to avoid being drowned, the arms after death were found rigid in the position they had before\*. I was told by a military surgeon that putrefaction appears very soon in the corpses of soldiers killed after the over-exertion of a prolonged battle. Thus all we know concerning man seems also in harmony with the law I am endeavouring to illustrate.

5th. *Influence of nutrition of muscles on cadaveric rigidity and putrefaction.*—Nysten† has shown that when death occurs accidentally in persons in health, in whom the state of nutrition of muscles is very good, and in whom therefore these organs are endowed with a high degree of irritability, cadaveric rigidity sets in long after death and persists very long. It is so, especially, in cases of death by decapitation, strangulation, asphyxia from non-toxic gases or submersion, by sudden hæmorrhage from a wound of a large artery, &c. Nysten states that he has seen cadaveric rigidity beginning only sixteen or eighteen hours after death, and lasting six or seven days. Dr. J. A. Symonds, in an excellent article on 'Death‡,' states that he has seen cadaveric rigidity eight days after death by hanging. My own observations show that in the bodies of healthy persons decapitated or asphyxiated, cadaveric rigidity does not appear sooner than ten or twelve hours after death, and that it lasts more than a week when the weather is not extremely warm. In the limbs of the two decapitated men on whose bodies I made experiments on the restoration of muscular irritability by injections of blood, I found that there was some degree of irritability thirteen or fourteen hours after death, before any blood had been injected. Nysten has seen muscular irritability lasting twenty-six hours in a decapitated man, which is a fact clearly showing that cadaveric rigidity may appear only after twenty-six hours.

The reverse is seen when nutrition has been diminished for a long while before death. In a man who died at the *Hôpital du Gros-Cailrou* at Paris, in the summer of 1849, cadaveric rigidity became evident within three minutes after the last breathing, and while the

\* Taylor's Manual of Med. Jurisprudence, p. 734.

† Recherches de Physiologie et de Chimie Pathol. 1811, p. 387.

‡ Cyclopædia of Anat. and Physiol. vol. i. p. 805.

heart was still beating twenty times in a minute, *i. e.* while the man was still alive, if life is considered to persist so long as the heart beats. These beatings ceased only three minutes and a half after cadaveric rigidity had shown itself everywhere. A quarter of an hour afterwards there was no more trace of cadaveric rigidity, and in less than an hour after death signs of putrefaction had appeared in the limbs. This man died of exhaustion after a prolonged typhoid fever.

It seems quite evident from the above fact, and others of the same kind which I have not mentioned, that the greatest differences exist as regards cadaveric rigidity and putrefaction in consequence of the degree of muscular irritability at the time of death.

6th. *Influence of convulsions on cadaveric rigidity and putrefaction.*—The law which it is my object to establish is borne out every day in cases in which death follows violent and prolonged convulsions. Sommer and other writers have seen cadaveric rigidity appearing so quickly after death by tetanus, that the spasmodic rigidity caused by the disease during life was followed by the *post-mortem* rigidity without any interval of muscular relaxation between these two states. In a strongly built woman, who died of hydrophobia after violent convulsions, I found that cadaveric rigidity had set in within the first hour after death, and that it ceased before the end of the tenth hour. In 1849 I made many observations at the *Gros-Caillou* Hospital in Paris, on corpses of soldiers who died from cholera, the general results of which are,—1st, that cadaveric rigidity appeared late and lasted long in those patients who died quickly, *i. e.* before a prolonged alteration of nutrition; 2nd, that those muscles which had been attacked with violent and frequent cramps, became rigid very soon after death, and remained so only a short time\*.

I have often had the opportunity of seeing rabbits dying from convulsions in cases in which the supra-renal capsules were the only organs found diseased, and have often observed in them that the more violent and frequent the convulsions, the earlier did cadaveric rigidity appear, the sooner it ceased, and the quicker also putrefaction occurred and proceeded.

7th. *Influence of certain poisons on cadaveric rigidity and putre-*

\* In one case cadaveric rigidity appeared in the calf of the right leg just after death, and ceased in less than two hours; while the other muscles, which had had no cramps, began to be rigid only five hours after death.

*faction*.—I do not intend examining any other influence of poisons except that which is due to the production of convulsions. I have made a great many experiments on this point, and the result has constantly been in harmony with the law now under consideration. The principal poisons employed were strychnine, nicotine, picrotoxine, atropia, morphia, oxalic acid, and the cyanide of mercury. I have ascertained that when death is produced almost at once by these poisons, there is hardly any effect to be observed on the time of access and duration of cadaveric rigidity and putrefaction; but when convulsions have existed for a long time before death, the influence is most remarkable. I will mention the results of but one experiment. Three dogs, as much alike as possible, and apparently in perfect health, were poisoned by the acetate of strychnine. One of them had a dose of 2 grains, another half a grain, and the third one-fourth of a grain. The first dog died at once, the second after 12 minutes, during 7 of which it had convulsions, the third after 21 minutes, during 11 of which it had convulsions.

	Durat. of musc. irrit.	Durat. of cad. rigid.	Putrefaction.
1st dog . . . .	8 hours	19 or 20 days	slow.
2nd dog . . . .	2½ „	5 days	rapid.
3rd dog . . . .	½ „	less than a day	very rapid.

In rabbits, guinea-pigs, cats and birds, as well as in dogs, I have ascertained that when they are killed by poisons causing convulsions, the more violent and the more frequent the convulsions are, the sooner cadaveric rigidity sets in and the less is the time it lasts; the sooner also does putrefaction appear, and the quicker is its progress.

I come now to the conclusion of this lecture. The facts I have mentioned show that when the degree of muscular irritability at the time of death is considerable, either in consequence of a good state of nutrition, as in persons who die in full health from an accidental cause, or in consequence of rest, as in cases of paralysis, or on account of the influence of cold, cadaveric rigidity in all these cases sets in late and lasts long, and putrefaction appears late and progresses slowly. The facts mentioned also clearly show that when the degree of muscular irritability at the time of death is slight, either in consequence of a bad state of nutrition, or of exhaustion from over-exertion, or from convulsions caused by disease or poison, cadaveric rigidity sets



in and ceases soon, and putrefaction appears and progresses quickly. The two distinct series of facts I have adduced seem, therefore, to bear out clearly the truth of the law I have endeavoured to establish on the relation between the degree of muscular irritability and the period of setting in and the duration of cadaveric rigidity, and also the time of appearance and the progress of putrefaction.

The Society then adjourned to Thursday, May 30th.

*May 30, 1861.*

Sir HENRY HOLLAND, Bart., V.P., in the Chair.

The reading of Dr. EDWARD SMITH'S Paper "On the Elimination of Urea and Urinary Water, in their relation to the Period of the Day, Season, Exertion, Food, and other influences acting on the Cycle of the Year," was resumed and concluded.

(Abstract.)

This communication contains the result of two principal series of inquiries which have been prosecuted by the author, viz. one upon himself, extending from January 1860 to March 1861, and comprehending 336 days; and the other upon four prisoners during the month of March 1860.

The former series included the amounts of urea, chloride of sodium, and urinary water evolved daily throughout the seasons, and their relation to period of the day, season, temperature, barometric pressure, and also to the cycle of the week, excess of food, and stomach derangement. The author also ascertained the effect of fasting from solid food and of drinking water, tea, coffee, and alcohol, in the absence of breakfast and during a whole day's fast. The net weight of the body night and morning was ascertained for many weeks in the early part of 1861, and the amount of fluid and solid food was recorded during many months of the year. During these inquiries the author lived in a moderate and regular manner, but without any prescribed dietary, and spent a part of August and

September at the sea side. He is æt. 42, weighs 196 lbs., and is in perfect health. The experiments upon prisoners were made in Coldbath-fields Prison, upon four prisoners in an average state of health, but below the average weight. The object was to determine the effect of treadwheel labour. The prisoners worked the wheel three days, and pursued routine labour on other three days in each week, whilst they rested on the Sunday. Their diet was good, and consisted of 20 oz. of bread, 6 oz. of cooked meat, 8 oz. of potatoes, 1 pint of cocoa, and 1 pint of gruel daily, besides a regulated quantity of water, which they drank at prescribed periods. They were under the immediate charge of an officer, and the greatest care was taken to ensure accurate results. On treadwheel days the urine was collected whilst the labour was performed and before breakfast was taken, and at other periods of the day, in a defined manner. The *feces* were weighed, and samples of the *feces*, urine and food, were kindly analysed by Mr. Manning. During the inquiry certain alterations of diet were made for periods of three days each, such as the administration of extra fat, tea, coffee, and alcohol, and the withdrawal of the chloride of sodium.

The analyses for urea and chloride of sodium were made by the author, and Liebig's method was adopted.

Both series comprised more than 1200 analyses for urea and 2000 collections of urine.

The following are the principal results obtained :—

1. The daily excretion of urea varied from 298 grs. to 748·5 grs., but on the average of the year was 519 grs., or 2·73 grs. to each lb. of body weight.

2. The extreme amounts of urine were 24·5 fl. oz. and 92·6 fl. oz., with a total average of 53·1 fl. oz. daily. The relation to body weight was 28 fl. oz. to each lb. There were noticeable daily alterations in the quantity, or the amounts proceeded in waves or arches, or progressively increased or decreased in a striking manner through a series of days under the influence of meteorological conditions and the statics of the body. The amounts derived from the cycle of the year are almost identical with the average of all recorded inquiries made for short periods, and upon various persons, and show the extreme value of including the cycle of the year.

3. The average quantity of urea in each oz. of urine was 9·4 grs.

This quantity increased with increase of urinary water above, and decreased with decrease of that fluid below the average; but the former in much higher proportion than the latter.

4. The average hourly emission of urea was on the whole day 21·7 grs.; during the night 16·5 grs.; before breakfast 20·3 grs.; and before midday 25·5 grs. The decrease of the night was 24, and of the early morning, or "basis quantity," 6·4 per cent., whilst the increase to mid-day was 17½ per cent. The "basis quantity" was so much influenced by the amount of urine passed on the previous day, that it was not a good standard of comparison for its own day.

The greatest increase of urea followed the breakfast, and the next followed the tea meal; whilst there was a considerable decrease at and after the hour of the early dinner. When an examination was made every ¼ of an hour, a maximum increase of 54·6 grs. of urea and 21 oz. of urine per hour was found.

5. There was the same general relationship of urinary water to the period of the day, but there was scarcely any increased evolution of that fluid in the afternoon.

6. Water taken several times during a day of fasting caused the same hourly progression in the elimination of urea and urine as occurs with food, except that the changes were more rapid and the decrease at the end of the intervals was greater. The maximum was 34·5 grs. of urea, and 11·5 oz. of urine.

7. When water was taken thrice before midday, and without breakfast, there was a large and rapid elimination of urea and urinary water; but when bread, tea, coffee, and black draught were added, the elimination was retarded. Alcohol with water, taken once before breakfast, caused within two hours an increase of 38 to 108 per cent. of urea, and 246 to 554 per cent. of urine.

8. With a daily dietary of bread and water, the hourly elimination was the same as with ordinary food; but when tea and coffee were added, there was a less morning and a greater evening increase of urea; but the amount of urine was altogether lessened.

9. There was the largest elimination of urea, carbonic acid, and fæces on the Sunday; and on that day there was much more rest, an increase of solid food, and a daily gain in weight of from 1 lb. 6 oz. to 2 lb. 3 oz.

10. The weight of the body was greatest on the Sunday, and lessened through a week of regular bodily exertion, but varied as the food and exertion varied. There were variations of several pounds recorded in each week.

11. The average weight of the solid ingesta during four months was 37 oz. in the week-day, and  $41\frac{3}{4}$  oz. on the Sunday; whilst the average weight of both fluid and solid ingesta was from  $89\frac{1}{4}$  oz. to 95 oz., and  $95\frac{3}{4}$  oz. at the same period.

12. The largest average elimination of urea and urinary water occurred from May to October (viz. 570 grs. and 480 grs. of urea), and the excess of the maximum over the minimum was 46.1 and 40.2 per cent.

13. The effect of temperature and atmospheric pressure was direct, and increased the elimination of urea and urinary water; but the former increase was commonly found on the succeeding day. When the two acted in the same direction, the common effect was increased; but when they were opposed they disturbed the result. Sudden increase of temperature lessened, and sudden cold increased the elimination of urea.

14. Treadwheel labour caused an increased elimination of 19 grs. of urea over that of days of routine labour, and 34 grains over that of the Sundays; but, in addition, the rate of elimination of urea to body weight was very high; viz. 4.39 to 4.74 grs. per lb.

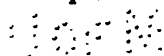
The labour of the treadwheel was equal to lifting 354 to 413 tons through 1 foot per day, or to walking about 29 miles daily. Sometimes the effect upon the urea was deferred until the following day.

15. Unusual and additional food always increase the elimination of urea; and, with headache and stomach derangement, there was commonly a temporary diminution in the excretion.

16. Tea, coffee, and alcohol, given to the prisoners, temporarily lessened the excretion of urea and urinary water, tea having the greatest effect upon the urea, and alcohol upon the urinary water; but this effect lessened after the first or second day. Tea increased the evolution of chloride of sodium, and alcohol decreased it proportionally with the urine.

17. When  $\frac{3}{4}$  oz. of chloride of sodium was withheld from the food, the same quantity was absent in the urine.

18. The amount of faeces was very large in the prisoners (8.55 oz.,



average), and contained 41·8 grs. of nitrogen daily. It was largest on the Sunday, and the fæces then contained an increase of nitrogen equal to the quantity lost by the urine on that day. Hence the assimilation was defective, but it was increased by labour.

The author then showed the relations of urea and carbonic acid, and their dependence upon food, season, and period of the day, and discussed the relation of urea to exertion and nutrition, showing that unless there be continued waste of the nitrogenous tissue, there cannot be any important increase of urea from exertion.

The paper was accompanied by numerous explanatory tables and curves.

The following communication was read :—

“On the Theory of the Polyedra.” By the Rev. T. P. KIRKMAN, M.A., F.R.S., and Hon. Member of the Literary and Philosophical Society of Manchester. Received May 10, 1861.

(Abstract.)

The complete resolution of the problem of the polyedra embraces the construction of all P-edra Q-acra, with an account of the symmetry of the solids when symmetrical. Such construction being impracticable from the magnitude of the task, it is desirable that a method should be found of classifying and enumerating the P-edra Q-acra, so that from this knowledge of the inferior polyedra, the same can be obtained concerning the higher, without any constructions, and without any tentative process.

I have found that all attempts to enumerate a separate and well-defined family of the P-edra Q-acra, beyond that I have called ‘The partitions of the R-pyramid’ (Phil. Trans. 1858), have been fruitless, and that the simplest method of solving the problem is to solve it entirely.

It is necessary first to have an accurate classification of polyedra as to symmetry. This is—

1. Zoned symmetry;
2. Zoneless axial symmetry;
3. Mixed symmetry, both 1 and 2;
4. Neuter symmetry, neither 1 nor 2;
5. Asymmetrical.

1. *Zoned symmetry*.—A polyedron may have zones of one, of two, or of three configurations. A zone divides the solid, when it is constructed with the greatest possible symmetry, into halves, of which one is the reflected image of the other. Zoned polyedra are,—

1. Monozones ;
2.  $m$ -zoned monaxines ;
3. Zoned triaxines ;
4. Zoned monarchaxines, having secondary axes ;
5. Zoned polyarchaxines, which have the axial systems of the regular polyedra.

The intersection of two zones is a *soned axis*.

The *sonal signature* gives an account of the number of zonal summit faces and edges, whether polar or non-polar ; but not of the number of edges in the zoned features.

2. *Zoneless symmetry*.—This is  $m$ -ple repetition of configuration, in revolution about a zoneless axis. An axis is *janal* if two opposite eyes can read at the poles configurations  $C C'$ , which are either identical, or one the reflected image of the other ; otherwise the axis is *heteroid*. A zoned janal axis is *objanal* when  $C$  is  $C'$  turned through two right angles. A zoneless janal axis is *contrajanal* when  $C$  is the reflected image of  $C'$ . The polyedra of this symmetry are,—

1. Zoneless heteroid monaxines ;
2. Zoneless monaxine contrajanal ;
3. Zoneless triaxines ;
4. Zoneless monarchaxine janals, having secondary axes ;
5. Zoneless polyarchaxines, having the axial systems of the regular polyedra.

3. *Mixed symmetry*.—In this there is both a zone or zones and one or more zoneless axes. The solids are,—

1. Monozone monaxines ;
2.  $m$ -zoned homozones, having  $m$  zoneless axes.

A zoned janal axis is *homozone* when the solid has zones of one configuration only ; otherwise the janal zoned axis is *heterozone*, i. e. the solid has zones of more than one configuration.

4. *Neuter symmetry*.—There is neither zone nor zoneless axis, but the edges of the solid fall into pairs  $ab, a'b'$  diametrically opposite, of which edges one is the reflected image of the other.

The polyedra of this symmetry are *contrajanal anaxine polyedra*.

5. The *asymmetric polyedra* are the most numerous of all.

The data required for the enumeration of the P-edra Q-acra are the following :—

A. A table of the hexarchaxine, tetrarchaxine, and triarchaxine P-edra Q-acra and Q-edra P-acra, both zoned and zoneless, with all the zonal signatures, and an account of the terminations of their principal axes, and with the *zonoid signatures*.

The *zonoid signature* in a zoneless or mixed symmetry gives the number of zoneless polar faces, summits, and edges.

B. A table of *janal poles*, containing an exact account of every polar feature terminating a janal axis, with the repetition if it be zoneless, and with the traces and zones if zoned.

C. A table of poles, containing an exact account of every polar feature terminating either a janal or a heteroid axis, with zonal signatures and traces when they exist, or with the zoneless repetition.

D. A table of faces and summits having a diagonal trace of a single zone, with the zonal signature.

E. A table of objanal monozone faces and summits, with their zonal and zonoid signatures.

F. A table of contrajanal anaxine pairs of edges, with the faces A B intersecting in them.

G. A table of all edges (A B) with their intersecting faces, the polar edges with their zones, the monozone zonal and epizonal edges each with its zone. An *epizonal edge* is cut by a zone.

These data being first obtained for the P-edra Q-acra and for the Q-edra P-acra, the numbers of the following solids and faces and summits are exactly known for both P-edra Q-acra and Q-edra P-acra ; and we suppose that all the inferior polyedra are registered in like manner :—

- a. The *m*-zoned monarchaxines with their zonal signatures, and their principal poles ;
- b. The zoned triaxines with their zonal signatures ;
- c. The *m*-zoned homozones with their zonal and zonoid signatures, and their exact principal poles ;
- d. The zoneless monarchaxine janals with their repetition and zonoid signatures, and their principal poles ;
- e. The zoneless triaxines ;

- f.* The monozone monaxines with their zonal signatures and their poles ;
- g.* The zoneless monaxine contrajanal with their repetition and their poles ;
- h.* The *m*-zoned monaxines with their zonal signatures and poles ;
- i.* The monozones, with their zonal signatures ;
- j.* The zoneless monaxine heteroids with their axes and repetition ;
- k.* The contrajanal anaxine polyedra ;
- l.* The asymmetric polyedra ;
- m.* The monozone A-gons and A-aces with their traces and zonal signatures ;
- n.* The objanal monozone A-gons and A-aces with their zonal and zonoid signatures ;
- o.* The janal anaxine A-gons and A-aces ;
- p.* The asymmetric A-gons and A-aces.

And these numbers being registered along with the data preceding for all signatures, we have a complete classification and enumeration of the P-edra Q-acra and Q-edra P-acra, which can be continued to any values of P and Q.

The difficulty lies in the obtaining of the data A B C D E F G. We begin with (C, D, and G).

*Analysis of a polar or monozone summit of a P-edron Q acron V.*

Let  $p'$  be a polar or monozone *p*-ace, which, if polar, is either a zoned or zoneless termination of an axis. If  $\frac{r}{2}$ -zoned or of *r*-ple zoneless repetition, there is a sequence of configuration *r* times read about the pole.

Any summit *m* through which lies the triangular section  $p'mn$  of the solid is a *deltotomous summit* about  $p'$  ; and if *mn* be an edge, it is a *deltotomous edge*, about  $p'$ , or of  $p'$ .

If the deltotomous edge *mn* be in two faces  $ff'$ , of which *f* has no deltotomous summits about  $p'$  except *mn*, while  $f'$  either has more than two deltotomous summits about  $p'$ , or is a triangle *mnr* having *r* collateral with  $p'$ , *mn* is a *primary effaceable* of  $p'$ .

If the deltotomous edge *mn* be in two faces  $ff'$ , of which neither has any deltotomous summits about  $p'$  besides *mn*, nor is a triangle *mnr* having *r* collateral with  $p'$ , *mn* is a *secondary effaceable* of  $p'$ .



The summit  $p'$  has  $e (\geq 0)$  effaced effaceables. These can be restored in one way only, since each  $(mn)$  must complete a face  $f$  which has no deltotomous summit besides  $mn$ . Let them be restored. We have the *perfect summit*  $p'$  (i. e. a summit which has no effaced effaceables) of a  $(P+e)$ -edron Q-acron  $V_p$ , which has  $E \geq e$  effaceables.

The process whereby the data C, D are obtained is the construction in groups of all *perfect* polar  $p$ -aces and monoxone  $p$ -aces having a diagonal zonal trace, of all the  $(P+e)$ -edra Q-acra  $(V_p)$ , which can be reduced by effacement of  $e$  effaceables to a polar or monoxone  $p$ -ace of a  $P$ -edron Q-acron  $V$ . These polyedra  $V_p$  can be all constructed about their perfect poles and registered; for no result of effacement is small enough to be employed in the construction of a polyedron  $(V_p)$ . Each perfect  $p$ -ace  $p'$  constructed has  $E (\geq e)$  effaceables, which are registered with the signatures of symmetry; so that all results of effacing  $e (\geq 0)$  of the  $E$  effaceables about any registered perfect  $p$ -ace are exactly known without repetition, and without enumeration of two summits, of which one is the reflected image of the other.

The process of this construction of perfect  $p$ -aces  $p'$  is the converse of the reduction of  $p'$ .

In the reduction of  $p'$ , we remove all the rays of  $p'$ , whereby we lay bare either a polyedron or a *reticulation*, which has among its linear sections the  $E$  effaceables of  $p'$ .

If the effaceables of the reticulation are all secondary, it is a *full reticulation*, which is an agglutination of polyedra cohering by those secondary effaceables, which are linear sections, and the only linear sections, of the reticulation.

If the reticulation has no effaceables, it is a *plane reticulation*, being simply a polygon partitioned by certain diagonals.

If there be one or more primary effaceables, we have a *mixed reticulation*.

The mixed reticulation always reduces in one way only by sections in its *external primary effaceables*, to a subject reticulation, which is either mixed or full or plane, or else to a polyedron. And, by continuing this reduction, we always obtain finally a full reticulation, a plane reticulation, or a polyedron,  $\frac{r_i}{2}$ -zoned, or of  $r_i$ -ple zoneless repetition.

The primary effaceables of a mixed reticulation are the joints or

seams by which it can be either taken to pieces or constructed, in one way only. And the portions into which it breaks up by these joints, are either polyedra, or full reticulations, or plane reticulations, of which we have an account in our register.

We *first suppose given* all plane and full reticulations, as well as the inferior polyedra, with all their signatures. By means of these, imposed as marginal charges on a subject reticulation, we can construct and register exactly all polar and monozone mixed reticulations, with their repetition and zonal signatures, and with their effaceables, which are always edges that have been loaded with marginal charges.

Every reticulation, plane, full, or mixed, is registered with its *marginal signature*.

The marginal signature exhibits to the eye all that is requisite to be known either for the *coronation* of the reticulation by a *p*-ace  $p'$ , whereby it becomes a polyedron, or for the construction on it of higher reticulations. The marginal signature to be constructed can always be exactly written; and there is a given number of ways of constructing it on the subject signature, which can be registered, and the number is obtained by inspection of our signatures. We always see in a mixed marginal signature what is removeable by sections in the external primary effaceables, and in a full reticulation we see what is removeable by external secondary effaceables.

The rules for charging a subject marginal signature, so as to construct another signature upon it, are:—

1. No primary effaceable which is external in the subject is to be external in the constructed; therefore the subject *compartment* standing on every external primary effaceable must receive at least one charge, which charge will be a *compartment* of the new signature.

2. Solid charges (polyedra or full reticulations) are imposed on plane marginal triangles of the subject. Plane reticulations, which give plane compartments, are imposed on solid marginal edges of the subject by a marginal triangle of the charge, which is lost in the operation, or supposed to be cut away. Thus a marginal triangle is lost by every charge imposed, whether plane or solid.

We handle all plane reticulations only by their marginal triangles, given by their signatures, and thus we escape the necessity of keeping any account of their summits.

3. A plane submargin of the subject is never charged.

4. Every charge adds a primary effaceable in a mixed reticulation, and may add any number of secondary effaceables, if a solid charge.

5. If the penesolid 402, which consists only of two marginal triangles, is employed to form a compartment of the new signature (which is the compartment  $(2)^r$ , or  $(2)$   $r$  times repeated in the  $r$ -ple marginal signature), the resulting reticulation is *closed* to further marginal increments, and can only be crowned.

The zonal and zonoid signatures of reticulations give an account of all zoned and polar effaceables, so that after coronation of a reticulation we can always enumerate the results of effacement, with their symmetry and signatures. Any effaceable can be effaced independently of others.

Asymmetric mixed reticulations are never constructed, unless such as can be crowned by a line; we thus escape the construction of the most numerous class of mixed reticulations. And asymmetric coronations, or asymmetric results of effacement, are never registered, unless the coronation be by a line, or the effacements be about a *perfect edge*.

An edge is a 2-ace summit which has always  $e(\geq 0)$  effaced effaceables that can be restored in one way only. When they are restored we have a perfect 2-ace, which is to be constructed and registered, whether symmetrical or not.

The removal of an edge lays bare in all cases either a polyedron or a *penesolid*, which may be either plane, full, or mixed.

A *penesolid* is a reticulation which can be crowned with a line, thus becoming a polyedron.

The order of the marginal features is not regarded in the marginal signature of a reticulation, as we keep no exact account of the faces about the crowning  $p$ -ace; but the marginal signature of a penesolid is always so written that the exact faces of the crowning line shall be known by inspection of the signature. A penesolid may have any number of effaceables which are registered with the marginal and zonal or zonoid signatures. The results of effacement about the crowning edges are the data CG, and these are exactly given by our methods.

The marginal signatures of penesolids are of the form

$[2a2b]$ ,  $[2a2b]$ ,  $[3a4c]$ ,  $[3aaAc]$ ,  
 $[2aAb]$ ,  $[AaBb]$ , &c.

The first has two marginal plane triangles and  $a + b$  submargins; the second has one marginal plane triangle; the third has none. The fourth has  $a + c$  plane submargins and  $a$  solid submargins. We see that polyedra are removeable by the external primary effaceables in the third, which have a  $(3 + 1)$ -gonal and a  $(4 + 1)$ -gonal face; and that the fourth has been constructed by charging the plane marginal triangles of a subject penesolid, one with a polyedron having a  $3 + 1$ -gonal face, and the other either with a polyedron having an  $(A + 1)$ -gonal face, or with a full penesolid having the solid margin  $A$ . The edge that crowns the first is the intersection of an  $(a + 3)$ -gon and a  $(b + 3)$ -gon. The edge that crowns the third may be the intersection of an  $(a + 3)$ -gon and a  $(c + 6)$ -gon, or of an  $(a + 4)$ -gon and a  $(c + 5)$ -gon, &c.

The polar summits being known for P-edra Q-acra and Q-edra P-acra, their reciprocal polar faces are of course known also.

This may suffice as an account of the data (C, D, G), except that we shall return to the construction of the plane and of the full reticulations.

We consider next the data B.

#### *Analysis of the polar janal summits $p'p_i$ of a P-edron Q-acron W.*

Let  $p'$  and  $p_i$  be  $\pi$ -aces terminating a janal axis. The ordinary effaceables of either summit, which are what we have already considered about a polar  $p$ -ace, can be restored in one way only. The janal symmetry remains, though modified by such restoration.

There are in faces about  $p'$  a certain number of triaces collateral with  $p_i$ . These are  $(w \geq 0)$  rhombotomous triaces. Let each be made collateral with  $p'$ , and let the  $w$  similar triaces in faces about  $p_i$  be made collateral with  $p'$ . They are now all rhombotomous tesseractes, such that quadrilaterals  $p' \rho p_i \rho_i$  are drawn through pairs  $\rho \rho_i$  of the tesseractes. We have now a pair of janal  $\pi'$ -aces perfect both in their ordinary and their peripolar effaceables, which is registered in our table of perfect janal poles.

A peripolar effaceable is any ray drawn from a principal janal pole to a rhombotomous tesseract, which is always collateral with either pole, and may lose either of its effaceables.

If the two polar summits be entirely removed, there is laid bare either a janal polyedron or a janal reticulation at either pole. The two opposite reticulations have the same marginal signature, which differs in nothing that needs here be noticed from that of a polar reticulation. This is reducible at either pole by the external primary effaceables, or if it be a janal full reticulation, by its external secondary effaceables, either to a janal polyedron, or to a janal full reticulation, or to a *primitive*, or to a *fundamental janal reticulation*; and this reduction is possible in one way only.

A *fundamental janal reticulation* is made thus:—Let a *primary polar plane reticulation*, i. e. one which has no diagonals but the bases of its marginal triangles, and no summits besides those of those triangles, be laid on a mirror, with the triangles a little raised from the mirror. The reticulation and its image form a *fundamental janal reticulation*.

If the primary be turned in any way about an axis perpendicular to the mirror, while the image remains stationary, we have a *primitive janal reticulation*. The edges in the mirror, common to both polar faces, are the *submargins* of the polar faces, on which primitive submargins rhombotomous tessaraces can be planted at pleasure in janal coronation.

The marginal triangles of the fundamental are all doubled, and the bases of the pairs are zonal effaceables. Some only of the marginal triangles of the primitive are doubled, and the bases of the pairs are zonoid effaceables.

There is only one way in which a given janal reticulation can be constructed on a fundamental or on a primitive. The rules for construction differ little from the rules for polar reticulations, and we conceive that the marginal charges are imposed alike in the opposite polar faces, thus preserving janal or contrajanal symmetry. Inspection of our signatures gives us the result in all cases.

The janal reticulation is registered with its marginal signature, which of course is the same at either pole, with its zonal signatures, principal and secondary, and with its zonoid signature if it have secondary zoneless poles.

An account is kept, in all constructions on a fundamental or primitive, of *primitive plane submargins*, because it is on these only that rhombotomous tessaraces can be deposited in janal coronation. These

are the submarginal edges (not in triangles) of the fundamental or primitive.

The symmetry of a janal subject is modified in various ways by marginal charges, by coronation, or by effacements ordinary and peripolar.

The modifications are the following:—

On the  $\frac{r_i}{2}$ -zoned heterozone janal subject we construct results,—

1.  $\frac{r}{2}$ -zoned heterozone,
2.  $\frac{r}{2}$ -zoned homozone,
3.  $r$ -ple zoneless monarchaxine janal,
4.  $r$ -ple monaxine monozone,
5.  $r$ -ple monaxine contrajanal.

On the  $\frac{r_i}{2}$ -zoned homozone subject we construct results,—

1.  $\frac{r}{2}$ -zoned homozone,
2.  $r$ -ple zoneless monarchaxine janal,
3.  $r$ -ple monaxine contrajanal.

On the  $r_i$ -ple zoneless monarchaxine janal subject we construct only  $r$ -ple monarchaxine janal results.

On the  $r_i$ -ple monaxine monozone subject we construct results,—

1.  $r$ -ple monaxine monozone,
2.  $r$ -ple monaxine contrajanal.

On the  $r_i$ -ple monaxine contrajanal subject we construct only  $r$ -ple monaxine contrajanal results.

In all these cases the constructions are given exactly by inspection of our signatures for every divisor  $r$  of  $r_i$ : and we can both complete the table of perfect janal poles of  $(P+e)$ -edra  $Q$ -acra which we require, and register the janal results of  $e$  ordinary effacements in every possible way upon each of them, for every kind of symmetry.

Having thus registered the results of *ordinary effacement* about janal axes in a table of janal poles perfect only in peripolar effaceables, we have next to register the results of *peripolar effacement*.

The effect of a peripolar effacement is, that a rhombotomous tessarace becomes a rhombotomous triace, and that the secondary zone,

if there is one, is destroyed, a different janal symmetry being introduced.

Our signatures give us an exact account of all rhombotomous tessaraces introduced in coronation, and we can readily enumerate the results of peripolar effacement by mere inspection of those signatures.

All fundamental and primitive reticulations are given by general formulæ, as are also the primary plane reticulations on which they are founded, in terms of their marginal signatures, and of their zonal and zonoid signatures. Every kind of janal symmetry may be seen in these fundamentals and primitives.

Thus, on the supposition, as before, that we have tables of inferior polyedra, and of the plane and full reticulations, symmetrical or not, we can obtain completely the data B for P-edra Q-acra, and for Q-edra P-acra.

The data E are what the  $\frac{r}{2}$ -zoned homozone polyedra become when  $r=2$ . They are given by the theory of construction of the homozone poles for the general value of  $r$ ; and each summit is registered with its zonal and zonoid signature and with its effaceables; and the results of effacement, ordinary or peripolar, are accurately known.

The data F are what the  $r$ -ple monaxine contrajanalns become for  $r=1$ . We never descend so low as  $r=1$ , except in the construction of penesolids, viz. contrajanal anaxine penesolids. The janal anaxine pairs which crown these and other penesolids are data F. They are registered as perfect edges with their effaceables, and the janal results of effacement are also data F.

The data A are easily obtained. Every polyarchaxine reduces in one way only to a regular polyedron, on the principal faces of which it is constructed, always by its principal poles.

The effaced effaceables of a polyarchipolar summit (which exists either on a polyarchaxine or on its reciprocal) are all ordinary, and can be restored in one way only. We conceive them restored about all like archipoles. These poles being removed, a polyedron or a reticulation is laid bare, which has a marginal signature differing in nothing that needs here be noticed from that of a polar reticulation. By inspection of this signature, we can construct on it a given number of polyarchaxine reticulations of given signatures, and the process differs in nothing from the construction of a polar reticulation, except

that we conceive it effected in all the principal faces of the subject. We can crown, and register all summits with zonal or zonoid and marginal signatures and with effaceables, so that the results of ( $e \geq 0$ ) symmetrical effacements about all the principal axes are readily enumerated. These results are the data A.

It remains only that we return to the construction of the full and plane reticulations, from which all our marginal charges, if they be not polyedra, are selected.

The full reticulations are reducible always either to a nucleus line or to a nucleus polyedron, by sections in their external effaceables. The marginal signature shows always the edges removeable by such sections, and construction proceeds by the rule that no external effaceable of the subject shall be external in the result. All effaceables are here secondary.

The modifications of symmetry of polar monozone and asymmetric subjects and constructions are expressed in general formulae, and the results are always registered with all signatures without ambiguity or repetition.

We never construct janal full reticulations except what have, as the word janal implies, a symmetry: and such constructions are always polar, except the objanal monozones and the contrajanal anaxine full penesolids. The only difference between these full constructions and that of mixed reticulations is, that no marginal triangles are handled or lost in the process; and that in the building of an  $r$ -ple repetition on a subject of  $ri$ -ple repetition, we descend to the value  $r=1$ , which gives the asymmetric full reticulations by the formulæ for the general value of  $r$ .

The plane reticulations have lastly to be considered.

All plane  $r$ -gonal penesolids ( $r0f$ ) having  $f-1$  ( $\geq 2$ ) diagonals, symmetrical or not, are given by general formulæ in terms of their marginal signature,

$$[2a2b], \quad (r=4+a+b),$$

that is, in terms of ( $rfa b$ ). The line which crowns this signature is the intersection of a  $(3+a)$ -gon and a  $(3+b)$ -gon.

All polar plane reticulations and all monozones which have less than three epizonal edges in the zone, reduce in one way only to a polar or monozone primary, which has no diagonals except the bases



of its marginal triangles. On this primary as subject the reticulations in question are constructed by one operation, by charging its marginal triangles with marginal triangles of inferior reticulations, whereby two marginal triangles disappear for every charge imposed. The number and the symmetry of the constructions are always given by inspection of our signatures. The monozones which have more than two epizonal edges in the zone, reduce by section in the central epizonal or epizonals to two or to three inferior zoned reticulations.

We construct, conversely, a given marginal and zonal signature thus in every possible way, the number of constructions being always given by inspection of our tables. All polar reticulations having this zone and marginal signature will be formed by the process. The polar being already registered, the monozones are obtained by subtraction.

Perhaps the greatest difficulty in the theory of the polyedra is the enumeration of the M asymmetric plane reticulations which have the marginal signature

$$S = [2^T w],$$

where  $2^T$  means simply  $2T$ ; and where  $T$  is the number of the marginal triangles, and  $w$  that of the submarginal edges, of which no two are contiguous. The reticulations to be found will be registered

$$R0F [2^T w] = M,$$

where

$$F - 1 - T = d$$

is the number of diagonals which are not bases of marginal triangles, and

$$R = 2T + s + u, \quad = 2T + w$$

is the number of the summits of the reticulation, of which  $s$  only are not summits in marginal triangles. If we erase the  $d$  diagonals, and also the  $s$  diaces that they may leave (summits of two edges), we have a primary reticulation,

$$R'0f [2^T u], \quad (R' = 2T + u),$$

which may or may not be polar or monozone. If now on the  $u$  submargins of  $R'0f$  we deposit  $s$  points (diaces) in every possible way, and then draw in the

$$(R - T =)(T + w =)(T + u + s)\text{-gon}$$

enclosed by the  $T$  marginal triangles  $d$  diagonals in every possible

way, none crossing another, so that one at least shall pass through each of the  $s$  points, we shall construct the asymmetric ROF in question among the results a certain number of times, namely, in every position of ROF in which erasure of the  $d$  diagonals and of the  $s$  diaces will reproduce the primary R'Of; and if R'Of be polar, there may be many such positions of ROF for the same position of R'Of. And it is evident that we shall construct equally every plane reticulation ROF of every symmetry which can reduce by the same process of effacement to the same primary R'Of.

Nothing is easier than to determine the number of asymmetric constructions thus obtained of ROF [ $2^T w$ ], if the number of all possible ways of drawing the  $d$  diagonals can be found.

We have to employ in turn every possible partition of the  $s$  points, of which one is

$$s = a_1 + a_2 + a_3 + \dots + a_m \quad (m \leq u), \\ (a_c \leq a_c + 1), \quad (a_1 > 0).$$

There is a given number of ways of depositing  $a_1$  points on any one of the  $u$  submargins,  $a_2$  points on any other, &c.

All that is difficult is to determine, when a disposition of the  $a_1 + a_2 + \dots + a_m$  points is made, in how many ways  $d$  diagonals can be drawn in the

$$(T + w) \text{-gon},$$

so that one at least shall pass through each of the  $s$  points. Let this number be

$$rd_{a_1 a_2 a_3 \dots}$$

It is given always by the equations following:—Let  $a_1 = 1$ ; then

$$rd_{a_1 a_2 a_3 \dots} = rd_{a_2 a_3 \dots} - (r-1)d_{a_2 a_3 \dots} - (r-1)(d-1)a_2 a_3 \dots$$

Let  $a_1 > 1$ ; then

$$rd_{a_1 a_2 a_3 \dots} = rd_{(a_1-1) a_2 a_3 \dots} - (r-1)(d-1)_{(a_1-2) a_2 a_3 \dots} - (r-1)d_{(a_1-1) a_2 a_3 \dots},$$

where

$$rd_0 = \frac{\Pi(r+d-1)\Pi(r-3)}{\Pi(r-1)\Pi(d+1)\Pi(r-d-3)\Pi(d)}$$

is what I have called in my memoir "On the  $k$ -partitions of the R-gon and the R-ace" (Phil. Trans. 1857), the  $(d+1)$ -divisions of the  $r$ -gon.

This number  $rd_{a_1 a_2 a_3 \dots}$  being thus given for every partition of  $s$ ,

the asymmetric plane reticulations  $ROF[2^Tw]$  are given, whatever be the primaries  $R'0f$ , symmetric or asymmetric, to which they are reducible by the above process of effacement.

And with these we have a complete solution, perfectly easy to calculate and register, of the problem of the *classification and enumeration of the P-edra Q-acra*.

The memoir, which I have the honour to present to the Royal Society, contains all the general formulæ of this solution.

There is nothing to prevent our registering in *all* marginal signatures the exact order of the margins. If this be done, we can crown every reticulation registered by a closed polygon A, made by connecting margins so that no linear section shall remain. The faces collateral with A would be at the same time constructed, and could be registered; that is, we could register all the faces collateral with any face in their order. And in crowning by summits marginal signatures so registered, we could determine and register the faces of every *p*-ace of the P-edra Q-acra in their order.

The methods above given are applicable to this more laborious registration of results. If this more tedious process be adopted, the construction of the P-edra Q-acra will be an easy matter.

I am not aware that anything has been printed on the subject of this theory beyond what I have attempted in the 'Philosophical Transactions,' and in the 'Memoirs of the Literary and Philosophical Society of Manchester,' except a short attempt made some three years ago by M. Poinso to sketch a beginning of the investigation, which appeared in the 'Comptes Rendus.' The attempt was well made, but the results given were not quite accurate. I have it not at hand; but I know that there is a defective enumeration of the simple solids there considered.

I have enumerated the polyedra of not more than 18 edges by this method, and I hope shortly to publish the classification and enumeration of the polyedra of 20 edges and under.

June 6, 1861.

The Annual Meeting for the Election of Fellows was held this day.

**GENERAL SABINE, R.A.**, Treasurer and Vice-President, in the Chair.

A letter was read from the President, intimating that, on account of the condition of his eyesight, he felt it to be his duty to decline being put in nomination for the Presidency at the next Anniversary.

The Statutes relating to the Election of Fellows having been read, Sir George Back and Mr. E. W. Brayley were, with the consent of the Society, nominated Scrutators to assist the Secretaries in examining the Lists.

The votes of the Fellows present having been collected, the following gentlemen were declared duly elected into the Society :—

Charles Spence Bate, Esq.

Heinrich Debus, Esq.

Campbell de Morgan, Esq.

Thomas A. Hirst, Esq.

A. Matthiessen, Esq.

J. Clerk Maxwell, M.A.

Ferdinand Müller, M.D.

William Newmarch, Esq.

Edmund Alexander Parkes, M.D.

William Pole, Esq.

Philip Lutley Sclater, M.A.

Charles F. A. Shadwell, Capt. R.N.

Henry J. Stephen Smith, M.A.

William Stokes, M.D.

George Johnstone Stoney, M.A.

June 13, 1861.

**THOMAS GRAHAM, Esq.**, Master of the Mint, V.P., in the Chair.

Mr. John Allan Broun, Dr. Augustus Matthiessen, and Mr. William Newmarch, were admitted into the Society.

The following communications were read :—

I. "Notice of Recent Scientific Researches carried on Abroad."  
By the **FOREIGN SECRETARY**.

Through M. Haidinger my attention has been called to the im-

portant labours of Professor Kolenati and Professor Oscar Schmidt, in Natural History, and to those of M. Albrecht Schrauf in Physics.

Dr. Friedrich Anton Kolenati, Professor of Natural History in the Polytechnicum of Brünn in Moravia, and Founder of the Bohemian Natural History Society 'Lotos,' is the author of upwards of 100 memoirs on various departments of Natural History, Geology, Mineralogy, and Crystallography, containing descriptions of nearly 400 species discovered by himself. The limits of this notice forbid the recital of the titles of these memoirs, or of the names of the species therein described. Some of Professor Kolenati's principal works are :—

*Meletamata Entomologica*, containing a description of the insects discovered by the author during a journey in the Caucasus and Transcaucasian provinces of Russia in the years 1843, 1844, 1845, 1846. Also various Memoirs on Chiroptera, Arachnida, and Insects, published independently or in the *Sitzungsberichte der Wiener Akademie*, *Lotos*, and the *Mémoires de la Société Impériale des Naturalistes de Moscou*, 1861.

Dr. Oscar Schmidt, Professor of Zoology and Comparative Anatomy at Gratz, is the author of the following memoirs :—

1. Die rhabdocoelen Strudelwürmer des süßsen Wassers. Jena.
2. Neue Beiträge zur Naturgeschichte der Würmer, gesammelt auf einer Reise nach den Färöer. Jena.
3. Neue Rhabdocoelen aus dem nordischen und dem adriatischen Meere. *Sitzungsberichte der Wiener Akademie*. Band IX.
4. Zur Kenntniss der Turbellaria rhabdocoela, &c. des Mittelmeeres. *Sitzungsberichte der Wiener Akademie*. Band XXIII.
5. Die rhabdocoelen Strudelwürmer aus den Umgebungen von Krakau. *Denkschriften der Wiener Akademie*. Band XV.
6. Die dendrocoelen Strudelwürmer aus den Umgebungen von Gratz. *Zeitschrift für wissenschaftliche Zoologie*. Leipzig. Band X.
7. Untersuchungen über Turbellarien von Corfu und Cephalonien. *Zeitschrift für wissenschaftliche Zoologie*. Leipzig. Band XI.
8. Ueber Planaria torva Autorum. *Zeitschrift für wissenschaftliche Zoologie*. Leipzig. Band XI.

The first five memoirs are devoted only to the arrangement of worms, Class "Turbellaria," which Ehrenberg named "Rhabdocoela." Subsequently the investigations are extended to the "Dendrocoela."

In No. 1 the general anatomico-physiological relations of the order are for the first time established with scientific precision, and a series of species are described accordingly. In No. 2 a very remarkable deviation of the genus *Dinophilus* is described. No. 3 contains the description of seventeen Rhabdoceles, which are all new with the exception of *Convoluta paradoxa*. In No. 4 are described nine species of Rhabdoceles from the Mediterranean, mostly from the Bay of Naples, together with some other worms. No. 5 contains the list and special description of twenty species, of which ten are new, observed in the neighbourhood of Cracow. The anatomical descriptions embrace almost all the principal genera: *Vortex*, *Dero-stomum*, *Mesostomum*, *Prostomum*. Nos. 6, 7, 8 contain researches respecting Turbellaria dendrocoela. The author has described most of the freshwater species existing in Germany, and some from Corfu.

Professor Schmidt's investigations extend to eighty species of the Turbellaria rhabdocoela, and dendrocoela, sixty-two of which were discovered by himself.

Since Quatrefages published his 'Mémoire sur les Planaries,' 1847, and Dalyell published his observations on various orders of Turbellariæ ('The Powers of the Creator,' 1851, 1853), nothing has been done either in France or England to improve the anatomical knowledge of this very attractive group. Professor Schmidt declares that he will consider it a reward for his own labours if English zoologists can be induced by means of this notice to turn their attention to the Fauna to which it refers.

The following notice of his researches has been furnished to the Foreign Secretary by M. Schrauf.

**Determination of the Optical Constants of Crystallized Substances. (First and Second Series.)** By ALBERT SCHRAUF (Vienna).

In the two hitherto published series of these investigations, the data concerning the refractive and dispersive powers of twenty crystallized substances are communicated.

Being persuaded that crystallo-physics, more than any other branch of physical science, is founded on quantitative calculation of absolute exactitude, I contrived to obtain first incontestable facts

connected with the hitherto somewhat neglected phenomena of dispersion and refraction. Nearly 1000 substances have been crystallographically investigated, and about 200 have been made the object of optical researches; many of them, however, have remained unknown as to their dispersive and reflective powers, which, representing the quantitative and qualitative action of any substance on the propagation of light, are of absolute necessity for the construction of any sound theory.

It becomes every day a greater necessity to obtain, within these extensive dominions of human knowledge, a certain number of general views, subservient to the explanation and systematic arrangement of a great number of isolated facts, as only a small portion of the present investigation has led to the establishment of general laws. The great problem of crystallo-physics proposed for solution may be expressed by the question, *What is the causal connexion between chemical constitution and morphological and optical properties?* The phenomena of isomorphism, discovered by Prof. Mitscherlich, have indeed thrown considerable light on the mutual relation of chemical constitution and morphological properties; yet little, if anything, has been done to arrive at the solution of the problem in its general form.

As latterly several doubts have been expressed as to the possible existence of such a connexion, the purpose of my investigations shall be not only (as expressed in their title) to fill up deficiencies in the knowledge of facts, but also to propose several explanations indicating the real existence of such a connexion, and the necessity of making it an object of earnest research.

In the following paragraphs I intend only to mention some theorems whose solution is already achieved. Another series of my investigations, to be published subsequently, is to afford general demonstrations and applications of consequences in strict connexion with duly stated facts.

The most important of the theorems, as far as they may be simply enunciated, are—

§ 1. The calculation, graphic representation, and derivation of all the crystallographical and physical properties of the *rhombohedral system* are possible if three rectangular axes are assumed; the axis *c* coinciding with the principal rhombohedral axis, and the axes *a* and *b* with the diagonals of the prism of  $60^\circ$ .

§ 2. The following indices represent consequently the characteristic equations for the *symmetrical* crystallographic systems:—

*Rectangular Axes.*

Tesseral  $a : b : c = 1 : 1 : 1$

Pyramidal  $= 1 : 1 : l$

Rhombohedral  $= \sqrt{3} : 1 : l$

Prismatic  $= h : 1 : l$ .

§ 3. The optical axes of elasticity, coinciding with the diagonal of the prism of  $60^\circ$ , are nearly equal to each other, and ( $\alpha$  and  $\beta$  being axes of elasticity and  $a$  and  $b$  crystallographical axes) if limit  $\frac{a}{b}$  is supposed to be  $= \sqrt{3}$ , then  $\alpha = \beta$ .

§ 4. Whenever a prism of  $60^\circ$  is extant in the *prismatic* system, the first median line ("bissectrice de l'angle aigu") is perpendicular to its diagonal\*.

§ 5. Whenever a number of prisms of  $60^\circ$  are extant (110, 011, 101), the first median line stands perpendicular to the diagonals of one of these prisms, and simultaneously to the plane of cleavage.

§ 6. The first median line is generally perpendicular to the diagonal of prisms, whose limit may be expressed by simpler proportions, as  $1 : \sqrt{2} : \sqrt{3} : \sqrt{5} : \sqrt{7}$ .

§ 7. The *dispersion* of the optical axes in the prismatic system is dependent on the magnitude of the crystallographical axis, with which the *middle axis of elasticity* is coincident.

(A) If the crystallographical axes ( $d_{2m}$  being a crystallographical axis, with which coincides the *second median line*  $d_\beta$ , with this the *medial axis* of elasticity being coincident) are to each other as limit of the square roots of *odd* numbers, then for

$$d_\beta > d_{2m} \text{ is } \rho > v,$$

$$d_\beta < d_{2m} \text{ is } \rho < v.$$

(B) If the same axes are to each other as the square roots of *even* numbers to the *odd*, then the law of dispersion becomes the *reverse* of what it was under the first supposition (A).

\* The totality of the substances belonging to the prismatic system, as far as they have been hitherto objects of optical investigation, may stand in proof and as confirmation of the propositions enounced here.



§ 8. *Hemimorphous* substances are dependent on the *opposite* law as to the dispersion of their optic axes.

§ 9. Whenever, according to Cauchy, the index of refraction  $\mu = A + \frac{B}{\lambda^2}$  (A being the coefficient of refraction and B the coefficient of dispersion), the relation between density D of the substances and the coefficients A and B is expressed by the following formulæ :

$$\int_{\lim D=0}^{D=\mu} 2A dA = \int_{\lim D=0}^{D=\mu} M dD,$$

$$\int_{\lim D=0}^{D=\mu} dB = \int_{\lim D=0}^{D=\mu} 2ND dD,$$

M and N remaining invariable quantities for every elementary substance. If M is made to signify *specific power of refraction*, and N *specific power of dispersion*, their values may be found out by means of the following equations :—

$$\frac{A^2 - 1}{D} = M, \quad \frac{B}{D^2} = N.$$

§ 10. The consequence from § 9 is, that the density of the *ether* may be set in proportion to the density of the *substances*\*.

§ 11. Not the *elasticity*, but rather the *density* is subject to variation (Fresnel's theory).

§ 12. The consequence of § 9 is, that the *propagation* of light may be equally conceived as being independent of the *luminous ether*, and only in dependence on the *substantial molecules*.

§ 13. If Fresnel's formula is derived from the principle of conservation of *vis viva*, and  $\frac{A^2 - 1}{D}$  is substituted, a formula similar to Cauchy's in structure is thus obtained.

§ 14. In consequence of § 9, it appears possible to calculate the *density* in the three dimensions of any crystal, and to bring this new moment in connexion with the rest of the physical properties.

§ 15. The *densities* being proportional to the *masses*, and these to the *distance r* of the *molecules*, the coefficient of *dispersion* must be subject to the general law of *gravitation*, and it would be admissible

\* Calcareous spar, Arragonite; diamond, graphite, coal; water, ice; different varieties of topaz, beryl, apatite, &c.; and all the substances examined by Dale and Gladstone may serve as evidences and exemplifications of the above propositions.

to write

$$\frac{B}{r^3} = N.$$

It must be remarked that, in consequence of the more or less relative mobility of the molecules,  $N$  must have a different value for different substances.

§ 16. The elementary substances, according to Boedecker's theory, not entering into mutual combination with the same density and correlation of atoms as they possess in the state of liberty, it may be admitted that they are probably compelled to form *double molecules*, or *semi-molecules*. In the second case of combination the *specific power* of *refraction* would be the *double*, in the *first* only the *half* of what it was in the elementary substance, when still chemically free.

§ 17. The indices of *refraction* of *chemical* combinations and mixtures may be calculated on the supposition enounced in § 16. Absolute exactitude may be obtained for any extensive groups, of which some members, and consequently the law of their chemical combination, are known\*.

The general law here is—Substances with nearly equal *powers* of *refraction* may combine without alteration in them. If these powers are very different, and far distant from each other, they tend to become homogeneous, so that the greater power of refraction is *halved*, and the lesser is *doubled* (see § 16).

§ 18. The coefficient of *dispersion* of combined substances† seems also to be derivable from the principles stated in §§ 9 and 16; as  $B$  being dependent on  $d^2$ , must change proportionally to the square of molecular condensation (see § 16).

§ 19. In consequence of § 9, the *lines* of Fraunhofer (although the constancy of the lengths of undulation, to which they answer, remains unaltered), when refracting substances are condensed, must advance their *centre* toward the *violet* end, in analogy to the phenomena of *absorption*, observed by Weiss (Poggendorff's 'Annalen,' 1861). For this reason *absorption* must depend on the length of the undulation.

\* Evidence is afforded by Dulong's, Dale's, and Gladstone's determinations, also by *all* other, especially organic, combinations, on account of the simplicity of their formulæ; also by Deville's, Grailich's, Handl's, and Weiss's determinations concerning mixed substances.

† Our knowledge of the coefficients of dispersion proper to chemical elements being still very defective, it is impossible to bring this proposition to full evidence. Sulphuret of carbon may serve for demonstration, and similar inferences may be deduced from investigations concerning mixtures.

These propositions, and some others of similar nature which I succeeded in deriving, incomplete as they still may be in many respects, prove at all events that, even in this department of human knowledge, theoretical points of departure exist from which, by progressive investigation, the real connexion of facts may possibly be traced. My next purpose is to coordinate as much as possible whatever is at present known of facts, bringing them under general points of view as materials for a future scientific theory. I intend to publish by and by these investigations as completely as possible, in constant connexion with the phenomena which they are suited to explain, never leaving the secure foundation of experimental research.

Here I intend only to offer a brief abstract of the facts obtained through the first two series of investigations, published in the 'Proceedings' of the Vienna Imperial Academy (vols. xli. and xlii.).

These investigations took place in the Imperial Physical Institute of Vienna, and could only be effectually accomplished through the liberal and kind assistance I received from the Director of the Institute, from the Director of the Imperial Museum of Mineralogy, and from the Superintendent of the Chemical Laboratory of the Imperial Geological Institute

In these results are comprised—the exponent of refraction, the calculated axial angle, the aperture of the cone of internal conical refraction, the measure of the apparent axial angle, and crystallographical investigations.

For brevity's sake I will only communicate the exponent of refraction for Fraunhofer's lines B and H, and the most important among the crystallographical determinations.

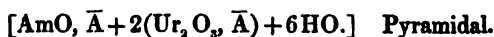
1. Diamond. C.

$$\mu_B = 2.46062 \quad \mu_H = 2.51425$$

2. Mellite.  $Al_2O_3, 3C_4O_3 + 18HO.$

$$\begin{array}{ll} \omega_B = 1.53450 & \epsilon_B = 1.50785 \\ \omega_H = 1.56113 & \epsilon_H = 1.52769 \end{array}$$

3. Acetate of Ammonia and Oxide of Uranium.



$$\begin{array}{ll} \omega_B = 1.47538 & \epsilon_B = 1.48770 \\ \omega_H = 1.50687 & \epsilon_H = 1.51974 \end{array}$$

4. Chloruret of Potassium and Cadmium.  $[2\text{KCl} + \text{Cd Cl}_2]$ Rhombohedral.  $a : c = 1 : 1.6483$ .

$$\omega_B = 1.58409 \qquad \epsilon_B = 1.58420$$

$$\omega_H = 1.62083 \qquad \epsilon_H = 1.62100$$

5. Chloruret of Ammonium and Cadmium.  $[2\text{NH}_4\text{Cl} + \text{Cd Cl}_2]$ Rhombohedral.  $a : c = 1 : 1.5704$ .

$$\omega_B = 1.59581 \qquad \epsilon_B = 1.59610$$

$$\omega_H = 1.64142 \qquad \epsilon_H = 1.64180$$

6. Nitrate of Soda.  $\text{NaO}, \text{NO}_3$ .Rhombohedral.  $a : c = 1 : 1.1903$ .

$$\omega_B = 1.57933 \qquad \epsilon_B = 1.33456$$

$$\omega_H = 1.62598 \qquad \epsilon_H = 1.34395$$

7. Nitrate of Potash.  $\text{KO}, \text{NO}_3$ .

$$\alpha_B = 1.49939 \qquad \beta_B = 1.49881 \qquad \gamma_B = 1.33277$$

$$\alpha_H = 1.54045 \qquad \beta_H = 1.53848 \qquad \gamma_H = 1.34359$$

8. Citric Acid.  $3(\text{C}_4\text{H}_5\text{O}_7, \text{HO}) + \text{HO}$ .

$$\alpha_B = 1.50542 \qquad \beta_B = 1.49432 \qquad \gamma_B = 1.48964$$

$$\alpha_H = 1.52541 \qquad \beta_H = 1.51398 \qquad \gamma_H = 1.50978$$

9. Sulphur.  $\text{S}$ . Prismatic.  $a : b : c = 1 : 0.5264 : 0.4279$ .

$$\alpha_B = 2.22145 \qquad \beta_B = 2.02098 \qquad \gamma_B = 1.93651$$

$$\alpha_H = 2.32967 \qquad \beta_H = 2.11721 \qquad \gamma_H = 2.01704$$

10. Quartz.  $\text{SiO}_2$  [for control].

$$\omega_B = 1.54106 \qquad \epsilon_B = 1.55012$$

$$\omega_H = 1.55806 \qquad \epsilon_H = 1.56758$$

11. Anatase.  $\text{TiO}_2$ .

$$\omega_B = 2.51118 \qquad \epsilon_B = 2.47596$$

$$\omega_H = 2.64967 \qquad \epsilon_H = 2.58062$$

## 12. Apatite. From Tumilla.

$$\omega_B = 1.63463 \qquad \epsilon_B = 1.63053$$

$$\omega_H = 1.65934 \qquad \epsilon_H = 1.65260$$

## 13. Beryl. From Elba.

$$\omega_B = 1.57028 \qquad \epsilon_B = 1.56540$$

$$\omega_H = 1.58884 \qquad \epsilon_H = 1.58261$$

From Graß Mogor, Brazil.

$$\omega_B = 1.57762 \quad \epsilon_B = 1.57148$$

$$\omega_H = 1.60321 \quad \epsilon_H = 1.59542$$

From Nertschinsk.

$$\omega_B = 1.56630 \quad \epsilon_B = 1.56165$$

$$\omega_H = 1.58818 \quad \epsilon_H = 1.58393$$

14. Carbonate of Lead. Cerussite.  $\text{PbO}, \text{CO}_2$ .

$$\alpha_B = 2.06131 \quad \beta_B = 2.05954 \quad \gamma_B = 1.79148$$

$$\alpha_H = 2.15614 \quad \beta_H = 2.15487 \quad \gamma_H = 1.86329$$

15. Formiate of Baryta.  $\text{BaO Fo O}_2$ .

$$\text{Prismatic. } a : b : c = 1 : 0.8638 : 0.7650.$$

$$\alpha_B = 1.63098 \quad \beta_B = 1.59181 \quad \gamma_B = 1.56788$$

$$\alpha_H = 1.66047 \quad \beta_H = 1.62176 \quad \gamma_H = 1.59643$$

16. Formiate of Lime.  $\text{CaO Fo O}_2$ .

$$\text{Prismatic. } a : b : c = 1 : 0.7599 : 0.4671.$$

$$\alpha_B = 1.57314 \quad \beta_B = 1.50997 \quad \gamma_B = 1.50669$$

$$\alpha_H = 1.59851 \quad \beta_H = 1.52971 \quad \gamma_H = 1.52577$$

17. Formiate of Strontia.  $\text{SrO}, \text{Fo O}_2 + 2\text{HO}$ .

$$\text{Prismatic. } a : b : c = 1 : 0.9477 : 0.8922.$$

$$\alpha_B = 1.53421 \quad \beta_B = 1.51743 \quad \gamma_B = 1.48057$$

$$\alpha_H = 1.55624 \quad \beta_H = 1.53769 \quad \gamma_H = 1.49899$$

18. Bimalate of Lime.  $\text{CaO}, 2\bar{\text{M}} + 9\text{HO}$ .

$$\text{Prismatic. } a : b : c = 1 : 0.9477 : 0.8922.$$

$$\alpha_B = 1.54037 \quad \beta_B = 1.50293 \quad \gamma_B = 1.48873$$

$$\alpha_H = 1.56500 \quad \beta_H = 1.52564 \quad \gamma_H = 1.51192$$

19. Ferrocyanuret of Potassium.  $3\text{KCy} + \text{Fe}_2 \text{Cy}_2$ .

$$\text{Prismatic. } a : b : c = 1 : 0.7725 : 0.6220.$$

$$\alpha_B = 1.57586 \quad \beta_B = 1.56151 \quad \gamma_B = 1.55913$$

[for D]  $\alpha_D = 1.58306 \quad \beta_D = 1.56888 \quad \gamma_D = 1.56596$

20. Asparagine.  $\text{HO}, \text{C}_6 \text{H}_7 \text{N}_2 \text{O}_5 + 2\text{HO}$ .

$$\text{Prismatic. } a : b : c = 1 : 0.8327 : 0.4737.$$

$$\alpha_B = 1.61392 \quad \beta_B = 1.57517 \quad \gamma_B = 1.54380$$

$$\alpha_H = 1.64221 \quad \beta_H = 1.60194 \quad \gamma_H = 1.56538$$

II. "Liquid Diffusion applied to Analysis." By THOMAS GRAHAM, Esq., F.R.S., Master of the Mint. Received May 8, 1861.

(Abstract.)

The unequal diffusibility of different substances in water appears to present means of separation not unlike those long derived from unequal volatility. For as regards diffusion, there exists a "volatile" and also a "fixed" class of substances; and these distinctions appear to correspond with differences in molecular constitution of a fundamental nature. Much value is attached to diffusion, as affording the means of bringing out clearly, and subjecting to numerical expression, the distinctive properties of what appear to be two great divisions of chemical substances.

The first, or *diffusive* class of substances, are marked by their tendency to crystallize, either alone or in combination with water.

When in a state of solution they are held by the solvent with a certain force, so as to effect the volatility of water by their presence. The solution is generally free from viscosity, and is always sapid. Their reactions are energetic and quickly effected. This is the class of *crystalloids*.

The other class, of low diffusibility, may be named *colloids*, as they appear to be typified by animal gelatine. They have little if any tendency to crystallize, and they affect a vitreous structure. The planes of the crystal with its hardness and brittleness are replaced in the colloid by rounded outlines with more or less softness and toughness of texture. Water of crystallization is represented by water of gelatination. Colloids are held in solution by a feeble power, and have little effect on the volatility of the solvent. They are also precipitated from their solution by the addition of crystalloids. The solution of colloids has always a certain degree of viscosity or gumminess, when concentrated. They appear to be insipid or wholly tasteless, unless when they undergo decomposition upon the palate and give rise to sapid crystalloids. Their solid hydrates are gelatinous bodies. They are united to water with a force of low intensity; and such is the character of the combinations in general between a colloid and a crystalloid, even although the latter may be a powerful reagent in its own class, such as an acid or an alkali. In their chemical re-

actions, the crystalloidal appears the energetic form, and the colloidal the inert form of matter. The combining equivalent of the colloid appears always to be high, and it has a heavy molecule. Among the colloids rank hydrated silicic acid, and a number of soluble hydrated metallic peroxides, of which little has hitherto been known; also starch, the vegetable gums and dextrin, caramel, tannin, albumen, and vegetable and animal extractive matters. The peculiar structure and chemical indifference of colloids appear to adapt them for the animal organization, of which they become the plastic elements.

Although the two classes are widely separated in their properties, a complete parallelism appears to hold between them. Their existence in nature appears to call for a corresponding division of chemistry into a crystalloid and a colloid department.

Although chemically inert in the ordinary sense, colloids possess a comparative activity of their own, arising out of their physical properties. While the rigidity of the crystalline structure shuts out external impressions, the softness of the gelatinous colloid partakes of fluidity, and enables the colloid to become a medium for liquid diffusion, like water itself. The same penetrability appears to take the form of a capacity for cementation in such colloids as can exist at a high temperature. Hence a wide sensibility on the part of colloids to external agents. Another eminently characteristic quality of colloids, is their mutability. Their existence is a continued metastasis. A colloid may be compared in this respect to water while existing liquid at a temperature below its usual freezing-point, or to a supersaturated saline solution. The solution of hydrated silicic acid, for instance, is easily obtained in a state of purity, but cannot be preserved. It may remain fluid for days or weeks in a sealed tube, but is sure to gelatinize at last. Nor does the change of this colloid appear to stop at that point. For the mineral forms of silicic acid, deposited from water, such as flint, are found to have passed, during the geological ages of their existence, from the vitreous or colloidal into the crystalline condition (H. Rose). The colloidal is in fact a dynamical state of matter; the crystalloidal being the statical condition. The colloid possesses *ENERGIA*. It may be looked upon as the probable primary source of the force appearing in the phenomena of vitality, as living matter without form. To the gradual manner also in which colloidal changes take place (for they always

demand time as an element), may the chronic nature and periodicity of vital phenomena be ultimately referred.

For the separation of unequally diffusive crystalloids from each other, jar-diffusion was had recourse to. The mixed solution was conveyed by means of a pipette to the bottom of a column of water contained in a cylindrical glass jar. A kind of cohobation takes place, a portion of the most diffusive substance rising and separating from the less diffusive substances, more and more completely, as it ascends.

The separation of a crystalloid from a colloid is more properly effected by a combination of diffusion with the action of a septum composed of an insoluble colloidal material. Animal membrane will serve for the latter purpose, or a film of gelatinous starch, hydrated gelatin itself, albumen or animal mucus. But by much the most effective septum used was paper, as it is metamorphosed by sulphuric acid (Gaine). It is now supplied by Messrs. De la Rue, and has become familiar under the name of "vegetable parchment" or "parchment-paper." From sheet gutta percha a flat hoop is formed, eight or ten inches in diameter by three inches in depth, and one side is covered by a disc of parchment-paper, so as to form a vessel like a sieve. A mixed solution, which may be supposed to contain sugar and gum, is placed upon the septum to a depth of half an inch, and the instrument then floated upon a considerable volume of water contained in a basin. Three-fourths of the sugar diffuses out in twenty-four hours, and so free from gum as to be scarcely affected by subacetate of lead, and to crystallize on the evaporation of the external water by the heat of a water-bath.

The unequal action of the septum, which causes the separation described, appears to depend upon this:—The crystalloid sugar is capable of taking water from the hydrated colloidal septum, and thus obtains a medium for diffusion; but the colloid gum has little or no power to separate the combined water of the same septum, and does not therefore open the door for its escape by diffusion, as the sugar does. This separating action of the colloidal septum is spoken of as *dialysis*.

Dialysis was applied to the preparation of various colloids. The mixed solution obtained by pouring silicate of soda into water acidulated with hydrochloric acid, was placed upon a parchment-paper



dialyser and allowed to diffuse into water, the latter being occasionally changed. After the lapse of five days seven-eighths of the original silicic acid was found to remain liquid upon the septum, and to be so free from hydrochloric acid and chloride of sodium as not to give a precipitate with acid nitrate of silver. The true hydrated alumina, and also Mr. Crum's metalumina, were obtained soluble by dialysing solutions of these oxides in the chloride and acetate of the same metal. So also the hydrated peroxide of iron, in addition to the hydrated metaperoxide of iron of M. Péan de Saint Gilles, and the soluble hydrated chromic oxide. The varieties of prussian blue are obtained soluble by dialysing their solution in oxalate of ammonia, the latter salt diffusing away. Stannic and titanic acids appear as insoluble gelatinous hydrates.

A solution of gum-arabic (gummate of lime), dialysed after an addition of hydrochloric acid, gave at once the pure gummic acid of Frémy. Soluble albumen is obtained in a state of purity by dialysing that substance with an addition of acetic acid.

Caramel of sugar, purified by repeated precipitation by alcohol and afterwards by dialysis, contains more carbon than any of the caramelic bodies of Gélis; it forms a tremulous jelly when concentrated, and appears decidedly colloidal. Caramel, like all other colloids, has a soluble and an insoluble modification. The latter has its solubility restored by the action of alkali, followed by that of acetic acid and subsequent dialysis.

Dialysis proves highly useful in separating arsenious acid and metallic poisons from organic fluids. Defibrinated blood, milk, and other organic fluids charged with a few milligrammes of arsenious acid, and placed upon the dialyser, were found to impart the greater proportion of the arsenious acid to the external water in the course of twenty-four hours. The diffusate was so free from organic matter, that the metal could be readily precipitated by sulphuretted hydrogen, and the quantity weighed.

Ice at or near its melting-point appears to be a colloidal substance, and exhibits a resemblance to a firm jelly in elasticity, the tendency to rend, and to redintegrate on contact.

The consideration of the properties of gelatinous colloids appears to show that osmose is principally an affair of the dehydration of the gelatinous septum under influences having a catalytic character, and

that the phenomenon is independent of diffusion. The colloidal septum is capable of hydrating itself to a higher degree in contact with pure water than in contact with alkaline solution. Colloidal septa, swollen in consequence of contact with dilute acid or alkali, appear to acquire increased sensibility to osmose, in consequence of their unusually high degree of hydration.

III. "On some new Phenomena of Residuary Charge, and the Law of Exploding Distance of Electrical Accumulation on Coated Glass." By Sir W. SNOW HARRIS, F.R.S. Received May 17, 1861.

(Abstract.)

A main object of this paper is to prove that residuary charge in the Leyden jar, subsequent to explosive discharge through an external interrupted circuit, as in the case of discharge by a Lane's electrometer, is not the result of a spreading of the charge upon the uncoated part of the glass, or of penetration within its substance, but arises from an undischarged portion of the accumulation left as it were behind, and still existing in precisely the same way and under the same conditions as the original charge.

The author introduces his subject with sundry observations on Lane's discharging electrometer, and the law of explosive discharge, and adverts to the fact recorded by Nicholson in the Royal Society's Transactions for 1789, that "although in moderate charges the exploding distance appears exactly, or very nearly, proportionate to the charge itself, yet for high intensity, the distance to which the charge is carried exceeds that proportion:" this the author finds to be the case generally, and quotes an experimental example showing the amount of deviation from Lane's law in that particular instance. He further shows, that in order to obtain explosive discharges at the increased distances agreeing with the calculated number of measures, the distances must be slightly increased by certain small quantities.

The probable sources of these differences are now adverted to, and the common objections to Lane's discharging electrometer considered. A new and improved form of this instrument is figured and described. One of its principal advantages is a means of changing the exploding points of the discharging balls, which are moveable on

axial centres, so as to bring a new point of the circumference into play, should abrasion or any other defect arise in the existing exploding point. The author endeavours to show that the apparent irregularities so frequently observed in the striking distance of a charged electrical jar, do not arise from any defect in the quantity measure, or in the exploding electrometer when properly constructed, but are altogether dependent on some peculiar conditions of electrical accumulation on coated glass.

One remarkable peculiarity of the electrical jar, is a disposition to retain a portion of the charge notwithstanding explosive discharge has occurred through a discharging circuit; we do not discharge the whole accumulation; a portion is, as it were, left behind. The fact itself is undisputed, but the cause or theoretical explanation does not appear to have been very clearly comprehended.

The author here introduces some interesting quotations from certain unpublished manuscripts of Mr. Cavendish, who investigated so long since as the years 1771 and 1772, what he terms the "charges of plate glass and other electrical substances coated in the manner of Leyden phials." Mr. Cavendish found his experimental inquiries greatly embarrassed by the "spreading of the electricity" on the glass; it is, he says, faster on some kinds of glass than on others; besides the slow and gradual spreading, he observed an instantaneous spreading, visible in the dark, and extending to about  $\cdot 07$  of an inch beyond the edge of the coating upon glass  $\cdot 2$  of an inch thick, and about  $\cdot 09$  upon glass  $\frac{1}{8}$ th of an inch thick. Another source of inconvenience, observes Mr. Cavendish, arises from a certain amount of penetration of the charge into the substance of the glass itself, equal to about  $\frac{7}{8}$ ths of its thickness; the space, he says, within which the charge cannot penetrate is not above  $\frac{1}{8}$ th of the thickness, from which he concludes that the charge of a coated electric will be different in cases in which this penetration of the charge into the substance of the glass varies, and infers that different electrics are susceptible of different degrees of charge. He examined plates of glass of various kinds, as also of gum-lac, rosin, bees-wax, &c., and found the capacity of these substances for electrical charge different—phenomena recently explained by Faraday's fine discovery of Specific Inductive Capacity.

This last celebrated philosopher also recognizes the penetration or

infiltration of electrical charge within the substance of coated and charged electrics, and attributes it to a certain amount of conducting power in the electrical substance. All substances, he infers, are conductors of electricity in a greater or less degree, and thus admit of infiltration of charge through their substance. In the case of charged electrics, the infiltrated electricity subsequently returns upon its path, and hence residuary charge.

The author has no disposition to question the experimental results arrived at by either of these eminent men, but is of opinion that they apply to a different case of electrical force than that of secondary and immediate discharge, supervening upon a primary discharge of an electrical jar, through an external explosive circuit, which he thinks can neither be referred to any previous spreading of the charge upon the glass, or to any penetration of it into its substance, or to return action as described by Faraday. He has found that of 100 measures of accumulated charge on a jar with imperfect conducting coatings, no less than 75 measures, or three-fourths nearly of the whole accumulation, has been left behind after explosive discharge. In a jar coated with water, full 14 measures out of 100, or about one-seventh, was left undischarged. He thinks it difficult to reconcile such an amount of residuary charge as this, with any spreading of the electricity on the glass, or any possible amount of penetration into its substance.

Although the deductions of Cavendish and Faraday may not be found to apply as solutions of the interesting problem of residuary charge, they still find their application in other cases, as in the case of the facts noticed by Nicholson already detailed. The intensity of explosive discharge may apparently become increased by a penetration of the exploding electricity into the air separating the balls of the discharging electrometer, in which case the measured distances of discharge, according to Cavendish, would, for given measured quantities of electricity, continually decrease, and discharge at the measured distances between the exploding balls would appear to happen prematurely. It is now shown by reference to a Table of experimental results, that at distances 1, 2, 3, 4, taken in tenths of an inch, with quantities of measured charge also as 1, 2, 3, 4, the actual distances of explosion are nearly as .1, .214, .325, .445. The author hence infers that, supposing the penetration of the first

measures very small and not of much value, the penetration of the succeeding measures may be taken as  $\cdot 014$ ,  $\cdot 025$ ,  $\cdot 045$ , that is  $\cdot 007$ ,  $\cdot 0125$ ,  $\cdot 0225$  upon each of the opposed exploding points, taking the surfaces of the exploding balls as curvilinear coatings to the intervening air.

If any considerable spreading of the charge upon the uncoated glass should arise, that, as remarked by Cavendish, would be equivalent to an increase of the coating, and hence the tension due to a given quantity of charge would be less. The effect would be greater on the first measured quantity than on succeeding quantities; hence for explosion at a first distance, an additional two or three measures might be required, which, as the spread upon the glass became satisfied, might not be requisite in the same proportion upon succeeding measured distances, in which case discharge would ensue with a less number of measures than calculation determines according to Lane's law, making it appear as if, according to Nicholson, "the intensity ran before the quantity."

Both Franklin and Nicholson have taken a sound practical and theoretical view of electrical accumulation on coated glass, which the author conceives to depend on a play of opposite electrical forces, either directly through the glass intermediate between the coatings, or through the medium of an external circuit, or both. He considers the terms "free" and "compensated," or "latent" electricity, perfectly admissible when correctly applied and limited by sound definition. All the accumulated charge, up to the exploding point, is evidently not sensible to the electrometer, and he thinks it convenient to distinguish between that portion of the charge of which the electrometer directly says nothing, and that portion to which its indications are more immediately referable, more especially as these two, or conjugate portions, have important relations to each other. Thus a double measured charge has twice the amount of free charge; and the free charge, as estimated by attractive force, is as the square of the accumulation. When the free charge explodes, the whole accumulation, or nearly all, goes with it, at least in common cases of metallic coated glass, and according to Nicholson carries it through distances proportionate to the charge itself: the terms "free" and "latent" electricity, or, as the French have it, "*électricité dissimulée*," may not be exact or admissible, if meant to imply a difference in kind

or mode of action of electrical force, but they are by no means objectionable when denoting different amounts of the same force in this or that direction.

In considering the nature of electrical accumulation on coated electrics and the law of explosive discharge, we have to deal with a simple question of physical force taken in the abstract, and not with a theoretical electric fluid or fluids of high elasticity, subject to expansion or contraction, changes in thickness of stratum, tension, density, and the like. The terms "tension" and "intensity," so commonly applied to designate degrees of electrical force, are convenient and not inappropriate terms when legitimately applied and limited by definition. The term *intensity* is well adapted to express the attractive force of the charge in the direction of the electrometer, and which, in continually *increasing* according to a known law, terminates in explosion. The intensity or attractive force varies with the square of the charge. The term "tension" is more especially applicable to the constrained state of the dielectric particles sustaining the induction necessary to the charge, and is equivalent to the reactive force of the particles in an interrupted circuit of discharge to break down or reverse the polarized state of the dielectric medium impeding discharge, as between the exploding balls of the *Lane's* discharger: this is as the quantity of charge directly. In employing these terms, the author has not the least view to any specific changes in the quality or condition of the accumulated electricity, as relating to density, elasticity, and such like. Whether the tension and intensity of a charge, as evidenced by the electrometer, be great or little, he conceives that the nature of the force and its mode of operation remains the same. Viewing the process of electrical accumulation and discharge in the Leyden jar as the result of certain powers or forces operating either immediately through the glass or through an external circuit, or both, we may readily imagine that at the critical point at which the forces in the two directions become balanced, and at which point the equilibrium of charge is, at it were, overset on the side of the exterior circuit, then it is that residual charge ensues, either by a momentary revulsion of force between the coatings in the direction of the intervening glass, frequently causing fracture, or otherwise by a retention of some of the charge in that direction at the instant of explosion. Some instructive and important experiments by Mr. T. Howdy are here quoted in support of this conclusion, from the

pages of the 'Philosophical Magazine' for the year 1815. A ruptured jar had the coatings removed from around the perforated part, so as to admit of the jar receiving a given amount of charge. When explosive discharge took place in the usual way, a spark was observed to pass at the same instant between the coatings through the perforation in the glass, evidently showing an exertion of force in that direction. This spark is entirely independent of the discharge in the circuit, the force of which remains the same as if no such perforation existed, as Priestley and other electricians, and Mr. Howdy himself, have fully demonstrated.

Considering the question of residual charge as bearing materially on our views of the nature of electrical force, the author seeks to investigate, by new forms and kinds of experiment, the relation of the residual quantity to the whole charge, whether accumulated on glass coated with very perfect conductors such as the metals, or otherwise with less perfect conductors, as water, or with imperfect conductors, such as paper, linen and the like. The instruments employed are now enumerated and commented on, and their experimental arrangement figured and described. They consist of the electrical or Leyden jar; Lane's improved electrometer; the hydrostatic electrometer as recently perfected; the thermo-electrometer; quantity or unit-measure; and battery charger and discharger. The following is the course which the author pursued in his inquiries, through the medium of these instruments.

The quantity of charge being given, its intensity is measured by the hydrostatic electrometer in terms of attractive force at a constant distance, suppose at distance 1 inch. This is first noted: the jar is now discharged through its exploding distance by completing the circuit through the Lane's discharger. The hydrostatic electrometer, being now made perfectly neutral, is again brought into connexion with the inner coating of the jar. The intensity or attractive force of the residuary or remaining charge is now noted, but as this force is necessarily small, it is taken with the attracting plates at a diminished distance from  $\cdot 1$  to  $\cdot 3$  of an inch or more, as the case may require, and subsequently reduced to the standard distance of one inch, taking the force to vary, as demonstrable by the electrometer, as  $\frac{1}{D^2}$ . This being determined, the relative quantities of electricity in the full charge and the residual charge will be as the square roots

of the respective attractive forces or intensities; the total force, as also demonstrable by the instrument, being as the square of the accumulation. Let, for example, the quantity of charge communicated to the jar be 100 measures, and the attractive force, or intensity at distance one inch, be 144 degrees, and suppose intensity of residual force at the same distance = .08. In this case we have the simple proportion 100 measures :  $x$  measures ::  $\sqrt{144}$  :  $\sqrt{.08}$  :: 12 : .283 and quantity of residual electricity =  $\frac{100 \times .283}{12} = 2.35$  measures

nearly; so that of the original 100 measures of charge communicated to the jar, rather more than  $\frac{1}{40}$ th remains undischarged in this case.

The author here offers some explanatory observations on the relative dimensions and extent of coating of the unit of measure and the relative value of the measures quoted, and he thinks if electricians would agree to recognize a standard instrument of this description, it would be attended with very considerable advantage, as in the case of other standard instruments. The unit of measure he employs exposes about 9 square inches of coating; it is about 4 inches long, .8 of an inch in diameter, and  $\frac{1}{40}$ th of an inch thick; distance of exploding balls .05 of an inch. Similar observations were applied to the thermo-electrometer, the ball of which is 4 inches in diameter, and has a wire of platinum through it of .01 of an inch in diameter. The dimensions of the attracting discs of the hydrostatic electrometer are also noted, which in these experiments were 4 inches in diameter; the suspended disc weighs 82 grains. The discs are carefully gilded; 5 degrees of the arc of measure represents a force of 1 grain, that is to say, a weight of 1 grain added to either side moves the index 5 degrees of the scale. Having offered these preliminary remarks, the author proceeds to the following experiments:—

*Experiment 1.*—Variable charges, amounting to 50, 100, 150 measures, were successively accumulated on different jars, exposing from two to six square feet of coating, and the residual charges due to each noted; these were found to be as the total charge. Thus the residual charge for 100 measures was in every case double that for 50 measures.

In a succeeding Table are noted—measured charge; exploding distance; intensity at distance 1 inch; residual measures and thermo-electric effect of discharge. It appears by this Table that residual



charge is as the total charge ; exploding distances, as the quantity or very nearly ; intensities and thermo-electric effect of discharge as square of the quantity or number of measure accumulated.

The author finds that for every metal-coated jar, whether large or small, of thick or thin glass, exposing from 1·5 to 6 feet of coating, the residual charge or quantity left undischarged, varies between the limits of  $\frac{1}{30}$ th and  $\frac{1}{80}$ th of the total charge.

*Experiment 2* investigates the effect of thickness of glass. Two jars, exposing 2·5 square feet of coating, were employed, their relative thickness being as 1 : 2, that is,  $\frac{3}{20}$ ths and  $\frac{6}{20}$ ths of an inch ; 100 measures were accumulated and discharged at their respective exploding distances. The following results appeared :—exploding distance directly as thickness of glass ; intensity or attractive force in direction of electrometer as square of the thickness ; residuary charge in each case the same, being about  $\frac{1}{45}$ th part of the total charge ; thermo-electric effect of discharge very nearly the same ; so that whether discharged from thick glass or thin, under intensities of very different degrees the same quantity of electricity produces the same effect. The intensities in this case were as 4 : 1, yet the thermo-electric effect did not differ more than one or two degrees, one being 12°, the other 13°. The author finds, by numerous experiments on a series of jars, that the intensity indication has no influence on the force of discharge, the quantity discharged being the same. In a series of jars of different magnitudes, and in which the intensity of a given charge of 100 measures varied between the limits of 100 and 1000 degrees, there did not appear a difference of more than a few degrees amongst the whole ; the effects varied between 8 and 11 degrees. Some little difference will generally arise in favour of electricity accumulated on a small area of coated glass ; in consequence of the greater facility of discharge the accumulation has greater freedom of operation through the external circuit, as is shown by its greater effect on the electrometer.

A celebrated electrician, the late Mr. Brooke of Norwich, in a conference with Cuthbertson about the year 1800, stated that a Leyden jar coated with strips of metal  $\frac{3}{4}$ ths of an inch wide, leaving intervals of the same width between the strips, was equally efficient as a full coating in the ordinary way. Two equal and similar jars, about 1 foot in diameter and 19 inches high, were prepared accordingly ; one fully coated to about 4 square feet, the other coated in strips

to about 3·5 square feet. The author, although doubting this statement in all its generality, still considered an investigation of it, more especially coming from such men as Brooke and Cuthbertson, desirable, and as being calculated to throw further light on the phenomena of the Leyden jar.

A few preliminary experiments seemed to accord with Mr. Brooke's view ; the exploding distance of the two jars with a given charge did not appear extremely different. The accumulated electricity spread upon the glass between the strips of metal, and thus enabled the partially coated jar to receive a larger accumulation, upon the principle stated by Cavendish, than was really due to its extent of actual coating. Mr. Brooke, in the then state of practical electricity, might have been therefore easily led to imagine that a partial coating such as he describes was sufficient. It is, however, shown in this paper that the cases of the two jars are widely different. As the spread of the electricity becomes satisfied, a less charge is required for explosion, and the tension of a given quantity increases. The following are the results of experiments with 100 measures similar to the preceding :—

*Full coating.*

Exploding distance . . . . .	·15	Intensity 100° at 1 inch.
Residual measures . . . . .	2·45	Therm. electric effect 8°.

*Partial coating.*

Mean exploding distance ..	·25	Intensity 160°.
Residual measures . . . . .	4·97	Therm. electric effect 3·5°.

It is evident the two forms of coating are not equally efficient, the heating effect of discharge not being half as great in the partially coated jar, whilst the residual charge is twice as great. The experiment so far shows the spread of electricity on the uncoated glass to be a source of absorption of charge to a greater or less extent, and goes far to confirm the views of Mr. Cavendish, relative to the spreading of electricity on glass.

The phenomena of metal-coated jars having been so far examined, a similar course of experiment is followed with jars coated with less perfect conductors, commencing with water coatings. For this purpose a jar exposing nearly 5 square feet of coating was prepared with metal coating, and the results of a charge of 100 measures determined

and noted as before ; the metal coating being removed, the same jar had an equal extent of water applied to its opposite surface coating. The method of effecting this is described. The author states that it was so perfect as to shield the experiment from all interference of vapour from the water surface, so that the jar completely retained the charge without any dissipation, and in no sense differed in this respect from a metal-coated jar.

The results of this experiment are not a little remarkable. The exploding distance of the 100 measures, whether with the metal or with the water coating, did not materially differ, except in apparent force, being for the metal  $\cdot 22$ , for the water  $\cdot 2$ . The exploding spark from the water coating, instead of the sharp ringing sound attendant on the exploding spark from the metal coating, is weak and subdued, and is often like the sound of fired damp gunpowder. The intensity or attractive force is also in each case alike, or very nearly ; being for the metal coating  $144^\circ$ , for the water  $142^\circ$ . The residuary charges differed considerably, being for the metal coating about  $2\cdot 25$  measures, or about  $\frac{1}{4\frac{1}{2}}$ th part of the total charge ; for the water coating  $14\cdot 5$  measures, or about the  $\frac{1}{7}$ th of the total charge. The residuary charge with a water coating is more than six times as great as with a metal coating. The thermo-electric effect with the metal coating was  $10^\circ$ , with the water coating nothing ; 200 measures, or double the charge, had no effect on the thermo-electrometer.

In this experiment it does not appear requisite that both the coatings should be water ; one coating may be metal, as in the first forms of the electrical jar. The author could not, at least, discover any material difference in the results, and concludes that if the first forms of the electrical jar with an internal coating of water had been continued, we should have had but small experience of the effects of artificial electrical discharge on metallic wires.

Imperfect conducting substances employed as coatings to the electrical jar have very similar but very exaggerated effects. With coatings of paper we have a striking example of retention of charge. A jar exposing  $5\cdot 5$  feet of coated glass, first coated with metal and subsequently with paper, gave the following results under a charge of 100 measures.

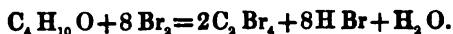
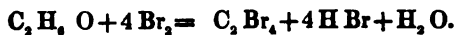
Exploding distances, as in the former case, nearly the same, being  $\cdot 23$  and  $\cdot 25$  ; attractive forces or intensity also nearly the same,

being 158° and 160°; residual measures with the metal coating 2·5 measures, or about the  $\frac{1}{40}$ th of the total charge; with paper coating, in some experiments 80 measures, or about  $\frac{8}{10}$ ths of the total charge, so that the residual charges with metal and paper are as 1 : 32. Thermo-electric effect for metal coating 8°, for paper coating nothing. It appears from these and similar experiments, that the interposition of imperfect conductors between the coating and the glass of the Leyden jar must necessarily impair its efficiency, and change its electrical indications, especially when of any considerable thickness. Three turns of common linen interposed between the outer coating and the glass reduced the force of discharge from 11° to 6°, nearly one-half, whilst the residuary or retention of charge is considerably increased: this question, as bearing in some degree on the retention of charge by the electric cable, may not be undeserving of further investigation.

IV. "On the Bromide of Carbon." By ARTHUR C. W. LENNOX, Esq. Communicated by Dr. HOFMANN. Received May 24, 1861.

The compounds of carbon with bromine have as yet received but limited attention. The tetrabromide of carbon,  $\text{CBr}_4$ , of the methyl-series is altogether unknown. Kolbe failed in obtaining this compound by submitting disulphide of carbon at a high temperature to the action of bromine, and I learn from Dr. Hofmann that the action of pentabromide of antimony on the disulphide gives no better result.

The bromide of carbon,  $\text{C}_2\text{Br}_2$ , has been obtained by Löwig\*, who procured this substance by the action of bromine on alcohol or on ether, when it is formed according to the equations—



These processes appear, however, to be attended with difficulties; at all events, Völckel†, who repeated Löwig's experiments, failed in obtaining the substance described by that chemist.

A simpler method for obtaining this bromide of carbon appeared to present itself in the perfect substitution of bromine for the hydrogen in olefiant gas.

\* *Annalen der Chemie und Pharmacie*, iii. 292.

† *Ibid.* xli. 119.

The brominated derivatives of ethylene have been chiefly examined by Regnault and by Cahours, and more recently by Wurtz and by Sawitch.

The following terms have been obtained :—

Dibromide of Ethylene.	Monobrominated Ethylene.
$C_2 H_4 Br_2$ .	$C_2 H_3 Br$ .
Dibromide of Monobrominated Ethylene.	Dibrominated Ethylene.
$C_2 (H_3 Br) Br_2$ .	$C_2 H_2 Br_2$ .
Dibromide of Dibrominated Ethylene.	Tribrominated Ethylene.
$C_2 (H_2 Br_2) Br_2$ .	$C_2 H Br_3$ .
Dibromide of Tribrominated Ethylene.	
$C_2 (H Br_3) Br_2$ .	

It thus appears that the whole series has been obtained with the exception of the tetrabrominated ethylene and its corresponding dibromide, the former of which would be the bromide of carbon obtained by Löwig in the reactions above indicated. Of the brominated derivatives of ethylene, dibromide of ethylene, monobrominated ethylene (bromide of vinyl) and dibromide of monobrominated ethylene, have as yet been best investigated. In re-examining these substances, in transforming dibromide of ethylene by the action of an alcoholic solution of potash into monobrominated ethylene, and the latter by treatment with bromine into the corresponding dibromide, I had an opportunity of tracing step by step the observations of my predecessors.

#### *Dibrominated Ethylene.*

The action of an alcoholic solution of potash upon the dibromide of monobrominated ethylene yields, together with secondary products, a comparatively small quantity of the oily compound  $C_2 H_2 Br_2$ , which is still further diminished by the extraordinary property exhibited by this substance of undergoing a molecular transformation similar to that of dichlorinated ethylene as observed by Regnault\*, and of monobrominated ethylene more recently examined by Dr. Hofmann†. Dibrominated ethylene having been lately the subject of a minute investigation by M. Sawitch‡, I have not entered upon a more

\* Annales de Chimie et de Physique, [2] vol. lxi. p. 151.

† Quarterly Journal of the Chemical Society, xiii. 68.

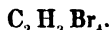
‡ Bulletin de la Société Chimique de Paris, 1860.

close examination of this body, but have at once, by the renewed action of bromine, converted it into the

*Dibromide of Dibrominated Ethylene.*

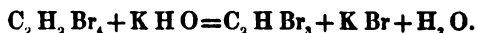
Bromine attacks the dibrominated ethylene with the greatest energy; heat is evolved, and the bromine disappears. When the bromine was no longer fixed, the orange-yellow liquid thus produced was treated with a weak solution of potash, washed with water, and ultimately distilled with precaution. It boils at about  $200^{\circ}\text{C}$ ., but not without partial decomposition; white fumes containing hydrobromic acid being given off during ebullition, which powerfully and persistently attack the eyes.

The bromine-compound is insoluble in water, but dissolves readily in alcohol and in ether. When exposed to a freezing mixture, it solidifies to a white crystalline mass. It was found impossible to purify this substance for analysis by distillation; a current of perfectly dry carbonic acid gas was therefore passed through the liquid heated in a water bath to about  $75^{\circ}$ . After a time it assumed a lighter colour, and no longer gave off hydrobromic acid; it was then found to contain



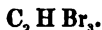
*Tribrominated Ethylene.*

The dibromide previously described is powerfully acted upon by an alcoholic solution of potash,



Addition of water to the alcoholic solution precipitates the tribrominated ethylene as an oily substance boiling at  $130^{\circ}\text{C}$ ., which by digestion with chloride of calcium and subsequent careful distillation, may be obtained sufficiently pure for analysis.

The results obtained in the analysis of this compound prove it to be represented by the formula



Tribrominated ethylene, like the other substitution-products of ethylene, is apt to furnish an isomeric solid, readily soluble in alcohol and in ether, from which it crystallizes in colourless plates. On slowly evaporating a solution of the oily modification in alcohol or ether, the crystalline solid is likewise obtained.

*Dibromide of Tribrominated Ethylene.*

To obtain this compound, tribrominated ethylene was introduced into a retort, and cautiously distilled into a receiver containing bromine. Combination took place with considerable evolution of heat. The orange-red liquid thus produced was washed with weak potash, and ultimately with water. Thus purified, this bromine-compound is a yellowish red oil, soluble in alcohol and in ether, and solidifying when exposed to a frigorific mixture. I have not succeeded in preparing this substance in a state of sufficient purity for analysis. But the nature of this body is clearly established by the facility with which it is transformed into

*Tetrabrominated Ethylene or Bromide of Carbon,*

by the action of an alcoholic solution of potash.

Addition of water to the alcoholic solution precipitates this compound as an oily liquid, which, when slowly deposited by spontaneous evaporation from its solution in alcohol or ether, separates in beautiful crystalline plates of an agreeable aromatic odour and burning taste. One or two recrystallizations render this substance perfectly pure. It is heavier than water, fuses at  $50^{\circ}\text{C}$ ., sublimates at a higher temperature, and is not attacked by the mineral acids.

These properties identify this compound with the bromide of carbon obtained by Löwig in the action of bromine upon alcohol and upon ether.

For analysis, the substance was dried over sulphuric acid *in vacuo*; the formula



was established by the determination both of carbon and of bromine. The tetrabrominated ethylene is generated by the action of an alcoholic solution of potash upon the dibromide of tribrominated ethylene, according to the following equation:—



The experiments which form the subject of this note were performed in the laboratory of Dr. Hofmann.

V. "On the Action of Dibromide of Ethylene on Pyridine." By  
JOHN DAVIDSON, Esq. Communicated by Dr. HOFMANN.  
Received May 24, 1861.

Pyridine, as is well known, has been obtained in the destructive distillation of coal, of certain varieties of shale, and of animal substances. The specimen with which I worked was obtained from coal-tar, and separated by distillation from the picoline: it boiled constantly at  $118^{\circ} \cdot 5$  C.

A mixture of pyridine and dibromide of ethylene gradually darkens, and at last becomes brown; no crystals, however, are deposited; on the other hand, the reaction proceeds with rapidity at  $100^{\circ}$  C. After the lapse of three hours the mixture is converted into an almost black crystalline mass, from which, by successive treatment with cold, and crystallization from hot, alcohol, a beautiful bromide, crystallizing in silky plates, is obtained. In preparing this bromide, it is advisable to add to the mixture of the anhydrous substances about one-fifth or one-sixth of its volume of alcohol, and to digest in sealed tubes. The whole liquid solidifies in this manner into a silky crystalline mass, which is only very slightly coloured.

The crystalline bromide is extremely soluble in water; I did not succeed in getting crystals from the aqueous solution. It is very soluble in boiling, but dissolves only slightly in cold alcohol; the boiling alcoholic solution solidifies on cooling into a pearly crystalline mass. Larger and transparent crystals may be obtained from dilute solutions, but in no case were their forms sufficiently perfect for determination. The simplest expression arrived at in analysing the bromide is the formula



but the mode of formation of the new compound unmistakeably shows that this expression must be doubled, and that the composition and weight of its molecule is represented by the formula



Pyridine, then, imitates triethylamine and triethylphosphine in their deportment with dibromide of ethylene, the new bromide being

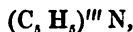
\* H=1; O=16; C=12, &c.



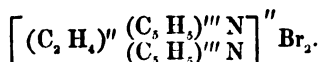
formed by the union of one molecule of the latter with two molecules of pyridine,



The constitution of pyridine itself is but imperfectly made out. All we know is that this substance is a tertiary monamine; the nature of the radicals which replace the hydrogen is as yet uncertain. Pyridine, in accordance with our present knowledge, may be represented by the expression

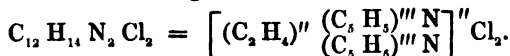


and the molecular construction of the new bromide may be expressed by the formula

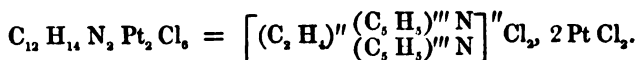


The composition of the bromide is confirmed by the analysis of the chloride and platinum-salt.

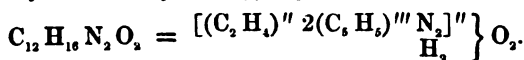
*Dichloride.*—Treatment of the dibromide with chloride of silver yields the corresponding dichloride as a crystalline and extremely soluble substance, containing



*Platinum-salt.*—The solution of the above salt yields with dichloride of platinum a pale yellow and apparently amorphous precipitate, insoluble in water and alcohol, and but slightly soluble in boiling concentrated hydrochloric acid, from which, on cooling, it almost entirely separates in small brilliant yellow plates, which were found to contain



The solution of the dibromide, when mixed with freshly precipitated oxide of silver in the cold, furnishes a transparent colourless and powerfully alkaline liquid which contains the corresponding base, viz. the hydrate of ethylene-dipyridyl-diammonium,



The existence of this compound in the solution is readily proved by saturating with hydrochloric acid, and adding dichloride of platinum, when the pale yellow platinum-salt previously mentioned is precipitated. This salt was submitted to analysis.

The hydrates of the diatomic pyridine-derivatives are far less stable than Professor Hofmann's ethylated bases in the nitrogen- and phosphorus-series. Even at the common temperature, much more so by ebullition, the solution of the free base becomes pink, violet, and finally ruby-red; and deposits after some time a brown powder, a peculiar odour like that of the heliotrope being at the same time evolved. I have not examined more minutely the changes which the pyridine-compound thus undergoes.

The above experiments were performed in Dr. Hofmann's laboratory.

VI. "On a New Class of Organic Bases, in which Nitrogen is substituted for Hydrogen." By PETER GRIESS, Esq.  
Communicated by Dr. Hofmann. Received May 24, 1861.

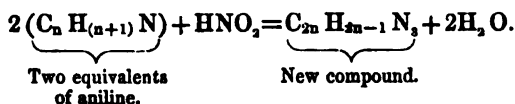
In a previous Note I have called attention to two new bodies produced by the action of nitrous acid upon the alcoholic solution respectively of aniline and nitraniline. Both substances\*,

Aniline derivative . . . .  $C_{12}H_{11}N_3$ ,

Nitraniline derivative . . . .  $C_{12}H_9(NO_2)_2N_3$ ,

are generated by the substitution of one equivalent of nitrogen for three equivalents of hydrogen in the original compounds.

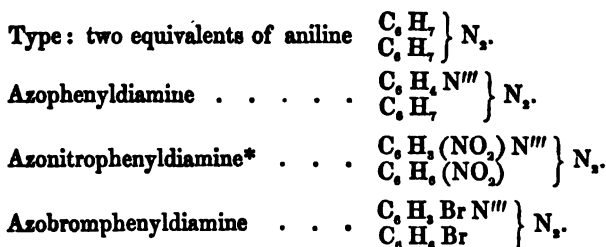
In continuing my experiments on the replacement of hydrogen by nitrogen in organic bodies generally, I have not only succeeded in producing similar compounds from nearly all the basic derivatives of aniline, but have also obtained corresponding products from toluidine and anisidine. The following equation represents the formation of these compounds in the aniline-series:—



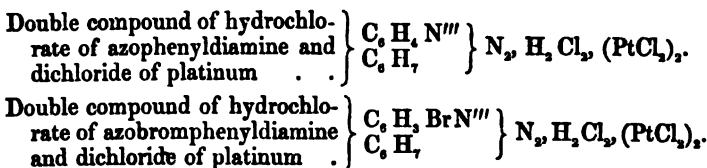
I do not at present venture to express an opinion regarding the constitution of these bodies; nevertheless their formation, their mutual relations, and their decompositions may in a measure be

\* H=1; O=16; C=12, &c.

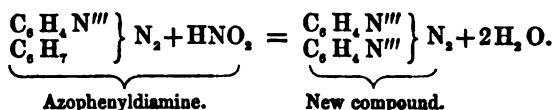
represented by the following formulæ:—



Nearly all these bodies are beautifully crystallized, and some possess very characteristic properties; the greater number are extremely weak bases, which it is scarcely possible to combine with acids. On the other hand, they readily produce double compounds with dichloride of platinum and trichloride of gold, the composition of which proves that these new bodies belong to the class of diatomic bases, as expressed by the following formulæ:—



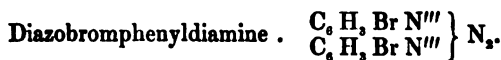
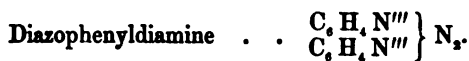
When exposed in alcoholic solution to the action of nitrous acid for a second time, most of these bases again lose three equivalents of hydrogen, which are again replaced by one equivalent of nitrogen, a new class of compounds being thus formed possessing in a still higher degree the peculiarities of the nitrogen-substituted bodies. The following equation illustrates this reaction:—



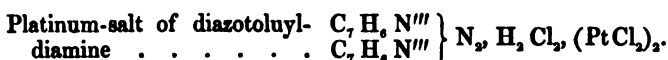
In practice, however, the bases are never obtained in the free state, but always as nitrates, from which they may be liberated by

\* This compound was obtained from the nitraniline discovered by Hofmann and Muspratt. I have already pointed out that a second body of the same composition exists, which is produced by a similar process from Arppe's nitraniline. Observations since made lead me to the conclusion that there is an isomeric representative corresponding to every compound of the phenyl-group.

the addition of an alkali. The following formulæ represent the bodies of this series which have hitherto been studied :—

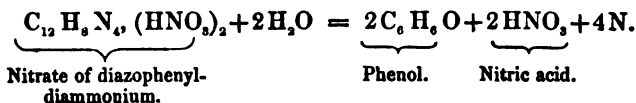


In the free state, these bases generally present themselves in the form of yellow precipitates, insoluble in water. On account of their comparative instability, they are little adapted for analysis; I have therefore generally fixed their composition by the analysis of their nitrates and platinum-salts, most of which are splendidly crystallized. The following formulæ represent several of these compounds :—



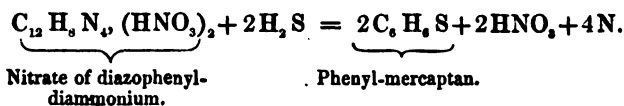
My attention has been especially attracted by this last class of new bodies, not only because of their peculiar constitution, but also on account of the remarkable physical properties by which, as a class, they are distinguished. One of their most striking features is the tremendous power with which, under the influence of heat or percussion, these substances explode. The greatest precaution is necessary in manipulating with these compounds.

I have scarcely commenced the study of the products of decomposition of these bodies. The following observations, however, justify the hope that they will not be without theoretical interest. Nitrate of diazophenyldiammonium, when heated with water, is rapidly decomposed according to the following equation :—



Qualitative experiments have proved that this substance undergoes

a corresponding transformation under the influence of sulphuretted hydrogen.



It is obvious that the last reaction supplies a general method for the production of the mercaptans belonging to the homologues of the phenyl-series, the first representative of which was discovered a few months ago by Vogt, who obtained it by submitting the chloride of sulpho-phenyl to the action of hydrogen.

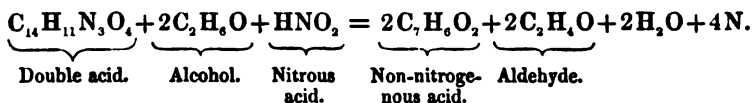
The observations described in this Note were made in Professor Hofmann's laboratory.

VII. "Reproduction of Non-Nitrogenous Acids from Amidic Acids." By PETER GRIESS, Esq. Communicated by Dr. HOFMANN. Received May 24, 1861.

In several previous communications I have pointed out the existence of a peculiar double acid,



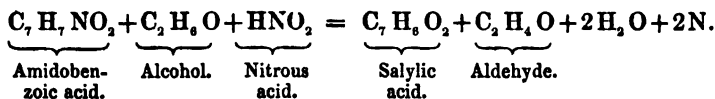
formed by the action of nitrous acid on amidobenzoic acid. This acid, which I have designated as *amidodiazobenzoic acid*, when again submitted in the presence of hot alcohol to the action of nitrous acid, is changed according to the following equation:—



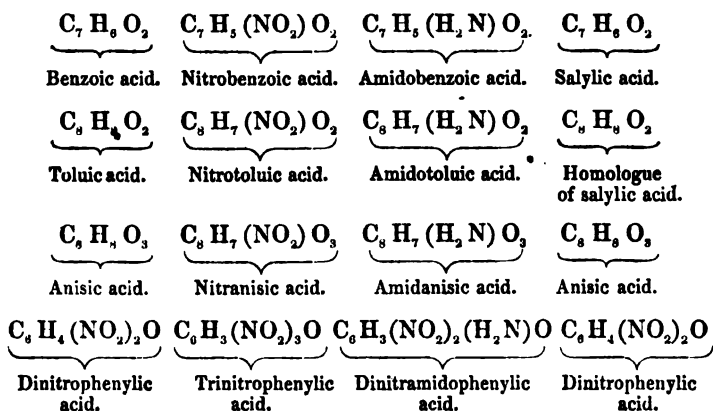
The non-nitrogenous acid thus reproduced exhibits the composition of benzoic acid; it differs, however, in its properties essentially from that acid. Since the publication of Kolbe and Lautemann's experiments on salylic acid, I have found that the acid in question is identical with the latter.

Salylic acid may be much more readily obtained by submitting an

alcoholic solution of amido-benzoic acid itself to the action of nitrous acid.



Nearly all the amidic acids I had an opportunity of examining exhibit a similar deportment, furnishing a non-nitrogenous acid isomeric or identical with the acid which, by conversion into a nitro-compound and subsequent reduction, had given rise to the formation of the amidic acid. In the following cases I have experimentally established this transformation.



It deserves to be noticed, that while amidobenzoic and amidotoluic acid furnish acids isomeric respectively with benzoic and toluic acid\*, anisic and dinitrophenylic acid are re-obtained with all their original properties. The properties of salylic acid, and of its derivatives, render it probable that salylic acid—and not benzoic as was hitherto assumed—is in the group of aromatic acids the true representative of acetic acid. Salylic, chlorosalylic, and salicylic acids exhibit in fact the same intimate relations which obtain between acetic, chloracetic, and glycolic acids. It is true the same relations may be traced in the composition of benzoic, chlorobenzoic, and oxyben-

\* The acid  $C_8H_7O_2$ , described by Strecker, probably belongs to a different group of compounds. In its transformations, and more especially in its conversion under the influence of oxidizing agents, into oil of bitter almonds, it differs both from toluic acid and the homologue of salylic acid.—P. G.

zoic acids, but the chemical deportment of these substances is altogether different from that of the analogous derivatives of acetic acid.

Further experiments are necessary to establish this view; the existence of two varieties of lactic acid, and of alanine and sarcosine, which are both represented by the formula



appears to indicate that similar isomerisms obtain also in the group of fatty acids.

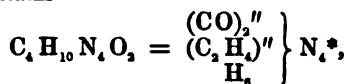
The above observations were collected in Dr. Hofmann's laboratory.

### VIII. "On the Ureas of the Diamines." By J. VOLHARD, Ph.D.

Communicated by Dr. HOFMANN. Received May 24, 1861.

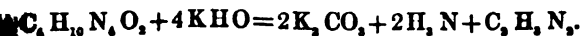
The action of cyanic acid and of the cyanic ethers on the monamines gives rise to the formation of the substances known as the *compound ureas*. By substituting diamines for the monamines in the above reaction, I have succeeded in producing a series of compound ureas of a higher order, which are remarkable for their well-defined properties and for their characteristic decompositions.

Dichloride of ethylene-diammonium is readily attacked by cyanate of silver. On evaporating the solution, separated from the chloride of silver, a new substance crystallizing in prisms is obtained, which I designate as *ethylene-urea*. This body is soluble in water and alcohol; it contains



and is formed by one molecule of ethylene-diamine combining with two molecules of cyanic acid.

Ethylene-urea fuses at 192° C. It dissolves easily and without decomposition in hydrochloric or nitric acid, without, however, combining with these acids; the acid solutions, when evaporated to dryness, leave the pure ethylene-urea. When boiled with a very concentrated solution of potassa, this body assimilates the elements of water and splits into carbonic acid, ammonia, and ethylene-diamine,



\* H=1; O=16; C=12, &c.

Ethylene-urea combines with dichloride of platinum and trichloride of gold.

The platinum-compound is obtained in short prisms of orange-red colour, containing



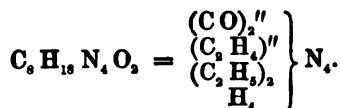
The gold-salt crystallizes in golden-yellow scales of the composition



The analysis of these compounds fixes the molecular value of the new urea. It deserves to be noticed, that this body, although derived from four molecules of ammonia, exhibits nevertheless in its combining power the character of but one molecule of ammonia; it is a *monacid tetramine*.

#### *Diethylated Ethylene-ureas.*

When cyanate of silver is treated with dibromide of ethylene-diethyl-diammonium, a reaction perfectly analogous to that observed between cyanate of silver and dichloride of ethylene-diammonium is accomplished. The solution, when separated from the bromide of silver and evaporated, solidifies on cooling to a crystalline mass. The new substance may be easily purified by dissolving it in absolute alcohol, from which it is deposited in the form of colourless needles, exceedingly soluble both in water and alcohol. The diethylated urea contains



This urea fuses at  $124^\circ$ , decomposing, however, partially at this temperature.

It forms a platinum-salt,



and a gold-salt of very little stability, which could not be analysed.

Cyanate of ethyl acts most energetically on ethylene-diamine. If the cyanate be poured into ethylene-diamine, each drop hisses like red-hot iron plunged into water. The mixture solidifies into a mass of needles, which, when recrystallized from water or spirit, furnish the



new compound in a state of purity. The substance thus obtained presents the same composition as the product of the action of dibromide of ethylene-diethyldiammonium on cyanate of silver, viz.

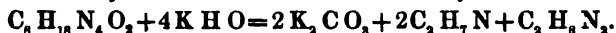


The two ureas, however, are far from being identical. The urea obtained by combining cyanate of ethyl with ethylene-diamine is easily soluble in boiling water, difficultly soluble in cold water, even less soluble in spirit, and almost insoluble in absolute alcohol. It fuses, without decomposition, at  $201^\circ$ , solidifying again at  $185^\circ$ . This body is altogether indifferent. I did not succeed in obtaining a platinum-salt or a gold-salt of this urea, while the isomeric substance, derived from ethylene-diethylamine, furnishes both salts without difficulty.

The non-identity of the two bodies is thus sufficiently established, but the absolute difference in their construction is most conspicuously manifested in the products of decomposition, which they yield under the influence of the alkalis. Treated with potash, the urea, derived from ethylene-diethyldiamine and cyanic acid, yields ethylene-diethyldiamine and the products of decomposition of cyanic acid, carbonic acid and ammonia :



The urea, on the other hand, which is formed by the action of ethylene-diamine on cyanic ether, splits into ethylene-diamine and the derivatives of cyanic ether, carbonic acid and ethylamine :



It is thus evident that the two individual groups of elements, the union of which gives rise to the two different ethylated ethylene-ureas, remain as such unaltered in these compounds.

The observations which I have recorded render it very probable that many of those substances known as compound ureas, and hitherto supposed to be identical, might on closer examination prove to be isomeric only.

Diethylurea, obtained by treating cyanate of ethyl with ethylamine or with water, differs indeed most conspicuously in its properties from diethylurea, produced by the action of cyanic acid on diethylamine. Under the influence of potash these two bodies split, the one into carbonic acid and ethylamine, the other into carbonic acid, ammonia, and diethylamine. Even the ethylurea, prepared by cyanic

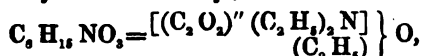
acid and ethylamine, differs in some of its physical properties from the ethylurea obtained by the action of ammonia on cyanic ether, although both substances yield under the influence of chemical agents exactly the same products of decomposition, viz. carbonic acid, ammonia, and ethylamine.

The experiments described in this Note were made in Dr. Hofmann's laboratory.

IX. "Notes of Researches on the Poly-Ammonias."—No. XI.  
Isomerism of Diatomic Compounds. By A. W. HOFMANN,  
LL.D., F.R.S. Received May 24, 1861.

The experiments which Dr. Volhard has described in the previous Note, remind me of some analogous observations which I made some time ago in the case of oxalyl-derivatives.

While engaged in experiments on the separation of the ethyl bases by means of oxalic ether, which I have had the honour of laying before the Royal Society \*, I had collected a considerable quantity of diethyloxamate of ethyl,

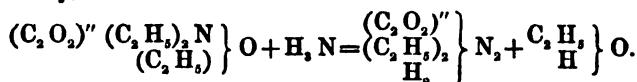


which is formed by the action of oxalic ether upon diethylamine. I have studied the deportment of this compound under the influence of several agents.

When digested with alcoholic ammonia in sealed tubes, diethyloxamate of ethyl is rapidly converted into a crystalline compound, which on analysis was found to contain



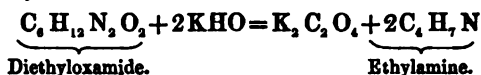
showing that the reaction is accomplished in the form pointed out by theory,



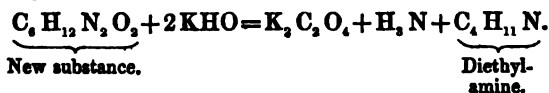
The new compound then has the composition of diethyloxamide, but it is far from being identical with the splendid body which is formed by the action of oxalic ether on ethylamine. It differs from the latter in many of its properties, being in fact much more soluble

\* Proceedings, vol. xi. p. 66.

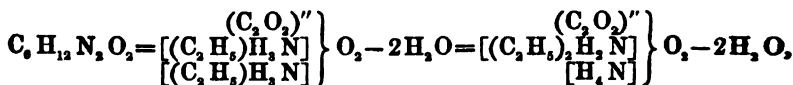
in water, and also by its transformations. Under the influence of hydrate of potassium the diethyloxamide of Wurtz splits into oxalic acid and ethylamine,



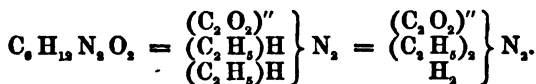
whilst under the same circumstances the new compound furnishes an oxalate together with ammonia and diethylamine,



It is thus seen that the isomeric diamides in general exhibit the relation which Dr. Volhard has so well developed for the ureas. The nature of these substances is determined by their genesis,



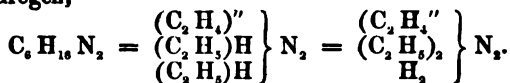
which is readily traced in their products of decomposition. The following formulæ might indicate the difference in the construction of the two bodies which form the subject of this Note,



It is obvious that similar cases of isomerism must obtain among the diamines or diatomic bases. Thus the formula



can represent two very similar bases, the constitution of which differs, inasmuch as the one owes its origin to the association of two molecules of ethylamine, while the other arises from the union of one molecule of ammonia and one molecule of diethylamine, the connecting link being in both cases the diatomic ethylene-molecule which replaces the hydrogen,

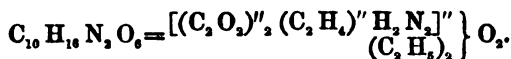


The facility with which the ethylated ammonias may be separated by means of oxalate of ethyl, has induced me to study the action of this ether on the diatomic bases. Ethylene-diamine, when treated

with oxalate of ethyl in the presence of alcohol, is converted into a compound crystallizing in long needles, to which analysis assigns the formula



This substance is the ethyl-ether of a diatomic amidic acid, *i. e.* the oxamethane of ethylene-diamine,

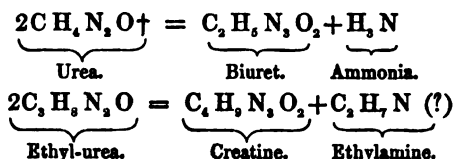


The action of monamines and diamines upon this compound gives rise to the formation of white insoluble bodies, similar to oxamide. I have not analysed these substances, the composition of which is sufficiently indicated by the logical development of the theoretical notions acquired in the study of the series of monatomic compounds. The action of oxalate of ethyl on ethylene-diamine produces, however, in addition, a new class of organic bases of a higher order, containing both ethylene and oxalyl. The examination of these bases, which are not represented among the monatomic compounds, will be the subject of a later communication.

#### X. "Notes of Researches on the Poly-Ammonias."—No. XII.

Action of Cyanate of Ethyl on Urea. By A. W. HOFMANN, LL.D., F.R.S. Received May 24, 1861.

The facility with which creatine, under the influence of chemical agents, furnishes urea or its products of decomposition, has led to many attempts at reproducing the flesh-base from urea. Weltzien\* has endeavoured to effect this transformation by submitting ethyl-urea to the action of heat, expecting that this compound would exhibit a change analogous to one of the phases of the transformation of normal urea, under the influence of heat,

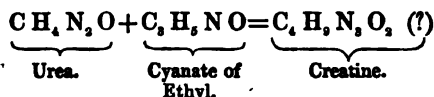


The reaction, however, appears to be accomplished in another form.

\* Ann. der Chem. und Pharm., c. 191.

† H=1; O=16; C=12, &c.

Creatine, or a substance isomeric with it, might have also been formed by the action of cyanate of ethyl on urea,



I have performed this experiment. Urea is most readily attacked by cyanate of ethyl. On heating, the former dissolves in the cyanate to a clear liquid, which, when submitted for a quarter of an hour in a sealed tube to a temperature of  $100^\circ$ , is entirely changed. The crystalline compound into which the liquid solidifies on cooling is no longer the original urea.

The principal product of this reaction is difficultly soluble in cold, but easily soluble in boiling water. The solution in hot water deposits on cooling beautiful white scales of silky lustre, which, by an additional crystallization, may be obtained in a state of perfect purity.

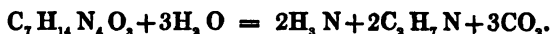
On analysis, this substance was found to contain



and it is thus seen that urea, instead of combining with one molecule of cyanate of ethyl, as is suggested in the above equation, fixes two molecules of this ether,

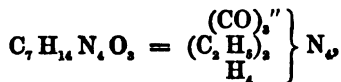


The new substance is readily soluble in alcohol and ether ; it likewise dissolves in dilute acids, but not more than in water ; it is also soluble in cold potash. Addition of an acid to this solution reprecipitates the original body without change ; ebullition with potash, on the other hand, destroys the substance, the products of decomposition being exactly what might be expected from a compound formed by the union of urea and cyanate of ethyl, viz. ammonia, ethylanine, and carbonic acid,

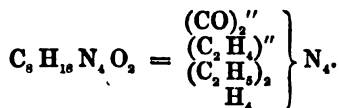


The formation of the new crystalline compound presents some interest in revealing the diatomic character of urea, which, uniting generally with one molecule of an acid, fixes in this case two molecules of (ethyl-cyanic) acid.

If we endeavour to form an idea regarding the constitution of this compound, it is obvious that it may be referred to four molecules of ammonia,

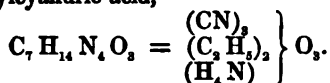


when it presents itself as a tetramine, as a urea of a higher order, and becomes in a measure analogous to some of the ureas of diatomic bases lately studied by Dr. Volhard\*, more especially to ethylene-diethyl-urea,



The latter compound differs from the crystalline substance produced by the action of cyanate of ethyl on urea by the presence of the diatomic molecule  $C_2H_4$ , in the place of the diatomic molecule CO.

The formula of the new compound represents also the ammonium-salt of diethylcyanuric acid,



The substance, however, is not an ammonium-salt *proprement dit*. Acids separate no diethylcyanuric acid; the alkalis in the cold evolve no ammonia; dichloride of platinum produces no precipitate in the aqueous solution.

The mother-liquor, from which the new compound has been deposited, contains cyanurate of ethyl and ethyl-urea, the formation of which substances requires no special explanation.

## XI. "Notes of Researches on the Poly-Ammonias."—No. XIII.

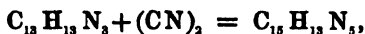
Derivatives of the Phenyl-Series. By A. W. HOFMANN, LL.D., F.R.S. Received May 24, 1861.

Some years ago I pointed out the existence of a peculiar class of bases, which are formed by the union of the aromatic ammonias with cyanogen. Only two of these compounds, cyaniline and dicyan-melaniline, have as yet been more particularly examined. I have of

\* See previous notice.

late had occasion to return to the study of these bodies, and to make an observation which I beg leave to communicate to the Royal Society.

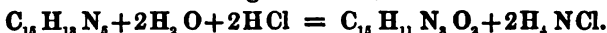
The product which is formed by the union of one molecule of melaniline and one molecule of cyanogen, the compound known as dicyanomelaniline,



has feebly basic properties; it dissolves in acids, and may be reprecipitated from these solutions without change. It cannot, however, remain in contact with acids without being entirely altered. After a few minutes the limpid solution becomes turbid, and deposits an indistinctly crystalline body containing



an ammoniacal salt remaining in solution,

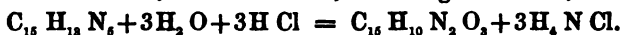


I have described this substance as *melanoximide*; it may be considered as the primary oxalate of melaniline *minus* 2 molecules of water, which are capable of being assimilated again under the influence of the alkalies, oxalic acid and melaniline being reproduced.

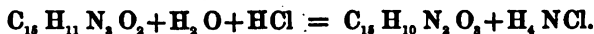
Dicyanomelaniline, when submitted in alcoholic solution to the action of acids, undergoes a perfectly different change. The boiling solution deposits on cooling splendid needles of an indifferent body of the composition



an ammoniacal salt, also in this case, remaining in solution,



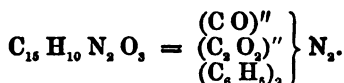
Melanoximide, occupying by its composition a place between dicyanomelaniline and the new compound crystallized in needles, is likewise readily converted into the latter substance. When boiled for a short time with a mixture of alcohol and hydrochloric acid, melanoximide furnishes a solution, which, on cooling, solidifies to a mass of beautiful needles,



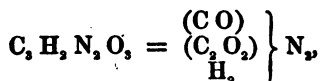
The new compound is insoluble in water, but dissolves easily in alcohol and ether; it is decomposed by boiling with potassa, giving rise to the formation of a carbonate and an oxalate with reproduction of phenylamine,



By this transformation the substance is characterized as the diatomic ammonia-derivative of phenylic alcohol, carbonic and oxalic acids, as *carbonyl-oxalyl-diphenyl-diamide*,

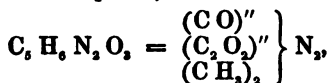


The new substance, both by composition and transformation, is closely allied to some compounds already known. The action of oxidizing agents on alloxan, and even on uric acid, has furnished to Liebig and Wöhler the substance known as *parabanic acid*,



which, by the action of alkalis, is converted into a carbonate, an oxalate and ammonia.

At a later period Stenhouse and Rochleder, when examining the products of decomposition of caffeine under the influence respectively of nitric acid and of chlorine, observed a crystalline compound (nitrotheine, cholestrophane),

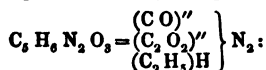


which, under the influence of alkalis, yields methylamine\* together with a carbonate and an oxalate, and may be viewed as *dimethylated parabanic acid*.

The compound which forms the subject of this Note, accordingly may be considered as *diphenylated parabanic acid*.

The transformations of dicyanomelaniline have suggested to me the study of the action of cyanogen upon some compounds having a composition somewhat analogous to that of melaniline. Thus the action

\* According to Rochleder, this compound, when boiled with potassa, yields *ammoniacal vapours*, in which he suspects the presence of *ethylamine*. The formula of the substance also represents an ethyl-compound,



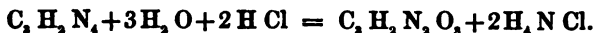
in this case ebullition with potassa should disengage a mixture of ethylamine and ammonia. But the fact that caffeine, when boiled with an alkali, evolves methylamine, renders it more probable that the caffeine-derivative is a methyl-compound. Experimentally the question remains undecided.



of cyanogen upon *cyanamide* might have yielded the compound



which, by acids, should have been converted into parabanic acid,



Cyanamide is, in fact, capable of fixing the elements of cyanogen, being converted into a yellowish amorphous powder, which, when heated with acids, furnishes a beautiful crystalline compound, difficultly soluble in water, and deposited from the boiling solution on cooling in long slender needles.

Cyanamide being very prone to molecular changes, when submitted to the action of chemical agents, I have not yet been able to procure a sufficient quantity of these crystals for a more minute examination. In their properties they entirely differ from parabanic acid.

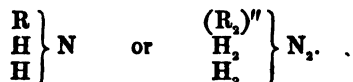
## XII. "Notes of Researches on the Poly-Ammonias."—No. XIV.

Diagnosis of Diamines. By A. W. HOFMANN, LL.D.,  
F.R.S. Received May 24, 1861.

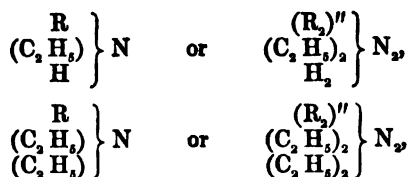
In former parts of this inquiry I have had repeated opportunities of discussing the features of distinction between the monamines and diamines. I have shown that the study of the genesis and of the transformations of an ammonia, and the observation of its boiling-point supply most valuable data for the elucidation of this question; and that the most reliable inferences may be drawn from the determination of its vapour-density.

In continuing my experiments upon this subject, I have been led to the discovery of a class of salts, the formation of which decides the question in an equally satisfactory manner.

Let us imagine an ammonia of uncertain origin, the composition and degree of substitution of which has been experimentally determined. It is obvious that as long as the boiling-point, and more particularly the vapour-density, remain unascertained, it is left doubtful whether the molecular value of the new compound is represented by

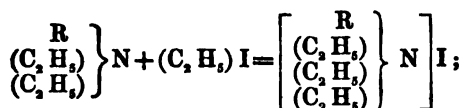


Nor is this doubt removed by the formation of ammonias of more advanced substitution. I have never succeeded in limiting the substitution to one equivalent of hydrogen in the case of diamines; the residual hydrogen in each of the linked ammonia-molecules is invariably affected in the same manner. Thus the action of iodide of ethyl upon the ammonia under consideration will produce two volatile bases,—



the atomicity of which is as uncertain as that of the body from which they are derived.

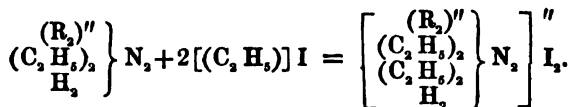
It is in the transformation of the ammonia into an ammonium-compound by the action of iodide of ethyl that the true nature of the substance is revealed. A monamine, thus treated, yields only *one* compound,



whilst a diamine gives rise to *two* compounds, viz. :—

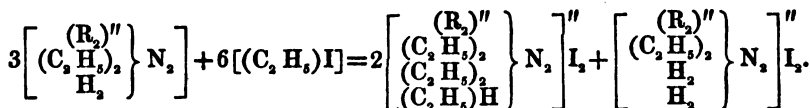


The first of these compounds is always formed when the secondary diamine is submitted to the action of iodide of ethyl. In this case two independent reactions are accomplished side by side. The first one transforms the diethylated diamine into a tetrethylated diammonium-compound,

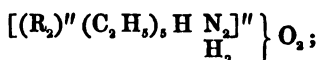
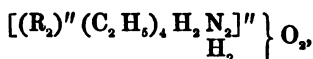
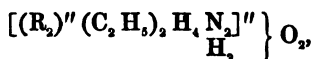


The second reaction gives rise to a *pentethylated* diammonium-

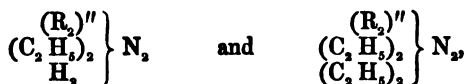
compound, while the iodide of the *diethylated* diammonium is reproduced,



Treatment of these mixed iodides with oxide of silver yields an alkaline solution containing the three bases—



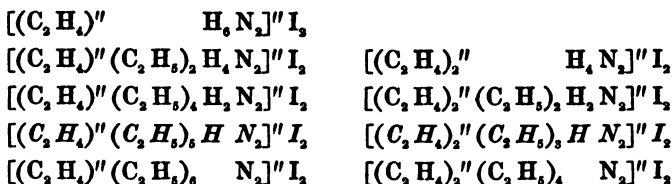
the two former of which, being capable of conversion into the volatile diamines



may be expelled from the solution by a current of steam, the non-volatile pentethylated diammonium-compound being left behind.

Treated again with iodide of ethyl, the pentethylated compound, lastly, is converted into the di-iodide of the hexethylated diammonium. *The formation of the pentethylated derivative establishes the diatomic character of the ammonia under examination.*

I have studied several non-volatile bases, retaining one equivalent of unreplaced hydrogen. The derivatives of the ethylene-ammonias deserve more particularly to be mentioned. Ethylene-diamine and diethylene-diamine, when submitted to the action of iodide of ethyl, give rise to the following series of compounds:—



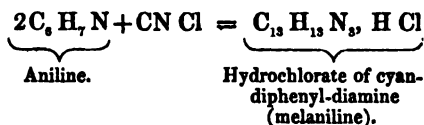
The diatomic nature of ethylene-diamine and diethylene-diamine is

sufficiently well established by their origin, and by the determination of their boiling-points and vapour-densities; so that the examination of non-volatile bases, retaining one equivalent of unreplaced hydrogen, is not necessary for the elucidation of their atomicity—affording simply additional evidence in favour of facts otherwise satisfactorily proved. But it is obvious how, in the case of compounds of uncertain origin, which may be easily decomposable or difficultly accessible, the formation of this class of salts must facilitate the diagnosis of the diamines, and may, under certain conditions, become the principal criterion for ascertaining the atomicity of an ammonia.

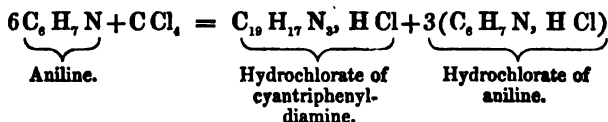
XIII. "Notes of Researches on the Poly-Ammonias."—No.

XV. Monacid Polyamines. By A. W. HOFMANN, LL.D.,  
F.R.S. Received June 13, 1861.

In former investigations I have pointed out the existence of a group of monacid bases containing three equivalents of nitrogen, the principal representatives of which, cyandiphenyldiamine and cyantriphenyldiamine, were formed by submitting aniline to the action respectively of chloride of cyanogen \*,



and tetrachloride of carbon,



I have lately met again with this group of bases in a new reaction which I beg leave to submit to the Royal Society.

When examining several years ago † the action of ethylate of sodium upon cyanate of ethyl, I observed the formation of triethylamine, but in subsequent experiments ‡ I could not reproduce this compound by

\* H = 1; O = 16; C = 12, &c.

† Proceedings of the Royal Society, vol. viii. p. 489.

‡ Journal of the Chemical Society, vol. xiii. p. 70.

this method, the reaction assuming a totally different form. I have not yet been able to elaborate the circumstances which determine the two different actions, but I have ascertained the nature of the substance which is formed instead of triethylamine.

Cyanate of ethyl, when allowed to flow in a slow stream upon perfectly pure ethylate of sodium, is powerfully acted upon; heat is evolved, which volatilizes a portion of the cyanate of ethyl; by far the larger portion, however, is converted into cyanurate of ethyl, which may be obtained with all its properties, simply by exhausting the dry cake in the retort, after cooling, with cold water.

On submitting the mixture of cyanurate of ethyl with ethylate of sodium to distillation at gradually increasing temperatures, ethylene gas is abundantly evolved, while a powerfully alkaline liquid condenses in the receiver, consisting of alcohol, an oily compound easily soluble in alcohol and difficultly soluble in water, ethylamine, and lastly, a powerfully basic oil, which boils at a very high temperature, at which it is partly decomposed.

The basic oil forms a series of saline compounds, amongst which the splendidly crystallized iodide, the beautiful platinum-salt, and the gold-salt deserve to be mentioned. The two latter, on analysis, were found to contain respectively,

Platinum-salt . . . .  $C_7H_{17}N_3, HCl, PtCl_2$ .

Gold-salt . . . . .  $C_7H_{17}N_3, HCl, AuCl_3$ .

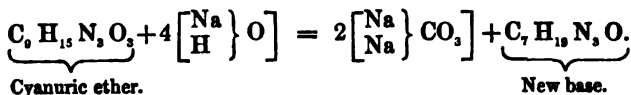
The composition of the base, such as it exists in combination with acids, is thus seen to be represented by the formula



In the free state, however, this substance retains with great pertinacity one molecule of water, being in fact

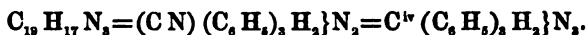


It owes its origin to the gradual action on the cyanurate of ethyl of the hydrated alkali, which is produced by the decomposition of the ethylate of sodium under the influence of heat,

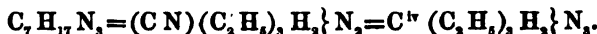


The interpretation of this new compound presents no difficulty.

A glance at the formula shows that it represents in the ethyl-series the body which in the phenyl-series I had previously formed by the action of tetrachloride of carbon on aniline. I have pointed out that the latter may be referred either to two or to three molecules of ammonia, and presents itself respectively as cyantriphenyl-diamine or as carbotriphenyltriamine,



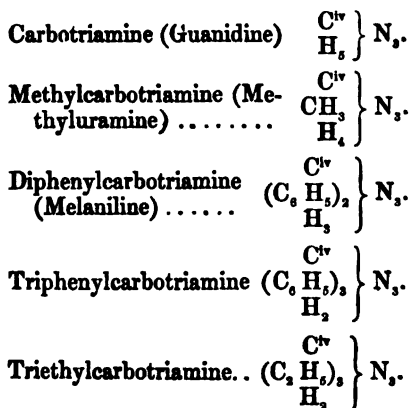
In a similar manner the new ethylated compound may be regarded as cyantriethyl-diamine or carbotriethyltriamine,



The latter formula would illustrate in some measure how the three ammonia-molecules are linked together in these compounds.

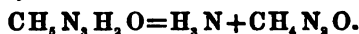
Whatever view may be taken of these bodies, whether they be regarded as diamines or triamines, they claim some interest on account of their well-defined characters and the numerous processes in which they appear to be formed.

Methyluramine, the base discovered by M. Dessaignes among the products of oxidation of creatine, belongs to the same group of bases, and perhaps the most interesting term of the series, guanidine, has been lately obtained by M. Strecker by the action of oxidizing agents upon guanine. Referred to three molecules of ammonia, the several bases which I have mentioned are represented by the following formulæ:—

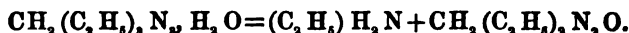


The perfect analogy of these several compounds is beautifully

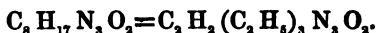
illustrated by the parallelism of their products of decomposition. Thus guanidine, when treated with nitric acid, is transformed into ammonia and urea,



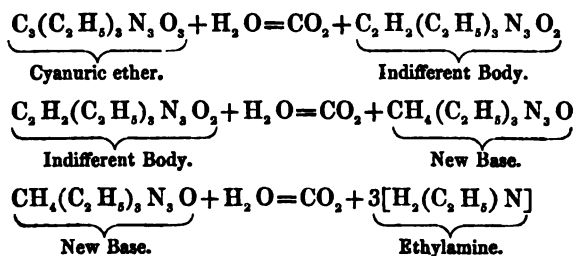
In a similar manner the triethylated compound, when submitted to distillation, splits into ethylamine and diethylurea,



The transformation of cyanuric ether into the new base presents additional interest, inasmuch as it exhibits the passage of this substance into ethylamine under a new aspect. MM. Habich and Limpricht have already pointed out that this transformation is by no means accomplished in one bound, an indifferent intermediate compound being formed, which from its products of decomposition was found to contain



The same oily compound accompanies the new base which is formed by the action of ethylate of sodium upon the cyanurate of ethyl. This substance stands midway between the ethylated base and cyanurate of ethyl, and it is thus seen that the latter passes through two intermediate stages before it is converted into ethylamine, the transformation consisting in the successive assimilation of three molecules of water, which determines the gradual elimination of the carbon of the cyanuric acid in the form of carbonic acid :



In the transformation of cyanuric acid into ammonia, obviously the same succession of changes must be distinguished. In fact the intermediate products are well known, although they have not as yet been obtained directly from cyanuric acid. The first product is the compound discovered by Wiedemann among the products of the

action of heat upon urea (cyanuric acid), and described by him as *biuret*; the second is the *guanidine* of M. Strecker. A glance at their formulæ,

Cyanuric acid . . . .  $C_3H_3N_3O_3$ ,

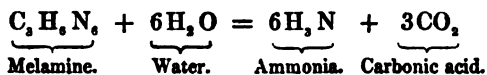
Biuret . . . . .  $C_2H_3N_3O_2$ ,

Guanidine . . . . .  $C_2H_5N_3O$ ,

shows that these substances are similarly related to each other as the derivatives of cyanuric ether, and appropriately selected methods will not fail to arrest the reaction at the intermediate stages.

Even now the cyanogen group is particularly rich in examples of serial transformations, accomplished under the influence of successively assimilated water-molecules. Liebig's celebrated researches have shown that melamine before it is converted into cyanuric acid undergoes a series of changes. Fixing water and losing ammonia in the same proportion, melamine is successively converted into ammelide, ammeline, and cyanuric acid. The latter, fixing water and losing carbonic acid instead of ammonia, passes through the intermediate stages, which I have endeavoured to trace, and is ultimately resolved into carbonic acid and ammonia.

The equation



represents the final result of the decomposition of melamine under the influence of the hydrated alkalies, but the water which figures in this equation is fixed, molecule after molecule, and the ultimate products are preceded by not less than five intermediate compounds, the successive formation and decomposition of which is illustrated by the following series of symmetrical equations:—

Melamine . .  $C_3H_3N_3 + H_2O = H_2N + C_2H_2N_2O$  Ammelide.

Ammelide . .  $C_2H_2N_2O + H_2O = H_2N + C_2H_4N_2O_2$  Ammeline.

Ammeline . .  $C_2H_4N_2O_2 + H_2O = H_2N + C_2H_5N_2O_3$  Cyanuric acid.

Cyanuric acid  $C_2H_5N_2O_3 + H_2O = CO_2 + C_2H_3N_3O_2$  Biuret.

Biuret . . . . .  $C_2H_3N_3O_2 + H_2O = CO_2 + C_2H_5N_3O$  Guanidine.

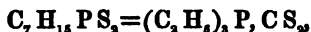
Guanidine . .  $C_2H_5N_3O + H_2O = CO_2 + H_2N_3$  Ammonia.

In carrying out the experiments, which form the subject of this Note, I have been most ably assisted by Dr. C. A. Martius, to whom my best thanks are due for his zealous and skilful co-operation.



XIV. "Researches on the Phosphorus-Bases."—No. XIII.  
 Sulphuretted Derivatives of Triethylphosphine. By A. W.  
 HOFMANN, LL.D., F.R.S. Received May 24, 1861.

In the former Notes of my researches on the phosphorus-bases, I have repeatedly alluded to the beautiful red crystals,



which are formed by the action of disulphide of carbon on triethylphosphine. I have pointed out that the facility and rapidity with which they are formed, render disulphide of carbon a most valuable reagent for the phosphorus-bases (trimethylphosphine exhibiting a perfectly similar deportment), since neither the monarsines nor the monostibines are in any way affected by this substance.

On the other hand, triethylphosphine may be employed with the greatest advantage as a test for disulphide of carbon. There is, in fact, no test for this substance which in delicacy could be compared with triethylphosphine. By its aid the presence of the disulphide in the most volatile fractions of coal-tar-benzol is readily proved; even the exceedingly small quantity of disulphide of carbon diffused in the most carefully purified coal-gas, may, as I have shown already in another place\*, be recognized without any difficulty.

In describing in a previous Note the formation and composition of the disulphide-of-carbon-compound, I have already mentioned the peculiar transformation which this substance suffers under the influence of water, being converted under these circumstances into the sulphide of triethylphosphine.

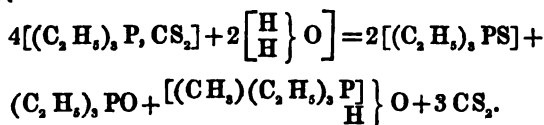
I have of late minutely examined this remarkable reaction.

The transformation is most conveniently studied by digesting the disulphide-of-carbon-compound with water in sealed tubes. After three or four days' exposure to 100°, the red crystals have disappeared, being replaced by the slender crystals of the sulphide. The latter compound is accompanied by several other products, the nature of which varies according to the duration of the reaction. In the first stages of the process considerable quantities of disulphide of carbon are separated, which in the latter stages may be traced in its products of decomposition, carbonic acid and sulphuretted hydrogen. The

\* Quarterly Journal of the Chemical Society, vol. xiii. p. 87.

mother-liquor of the sulphide of triethylphosphine is distinctly alkaline, and yields, after the necessary operations, on addition of dichloride of platinum, the well-known octohedral platinum-salt of methyl-triethyl-phosphonium. On further evaporating the liquid from which these crystals had been obtained, the six-sided tables of the platinum-salt of oxide of triethylphosphine were deposited.

The principal products of the action of water upon the red crystals, then, are sulphide of triethylphosphine, oxide of triethylphosphine, hydrate of methyl-triethylphosphonium, and disulphide of carbon, which may be partly or entirely converted into sulphuretted hydrogen and carbonic acid. Four molecules of the disulphide of carbon-compound, and two molecules of water, contain the elements of two molecules of the sulphide, one molecule of the oxide, one molecule of the hydrated phosphonium, and three molecules of disulphide of carbon :—



Whilst engaged with the experiments involved in the elucidation of this subject, I observed occasionally small well-defined yellow crystals disseminated among the mixture of white and red needles, which were deposited when the digestion-tubes were allowed to cool before the transformation was terminated. The yellow crystals appeared in greater quantity towards the close of the operation, but were even under the most favourable circumstances obtained only in extremely minute quantity.

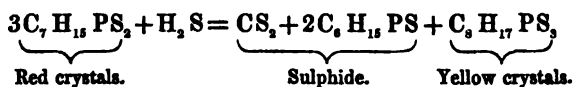
These crystals are almost insoluble in ether, and may therefore be readily separated from the sulphide of triethylphosphine with which they are contaminated. Crystallization from boiling absolute alcohol furnishes them in a state of perfect purity, occasioning, however, a certain loss, which may become very considerable if ebullition be continued for some time. This substance is decomposed at 100°, and had therefore to be dried for analysis, *in vacuo*, over sulphuric acid.

The yellow crystals were found to contain



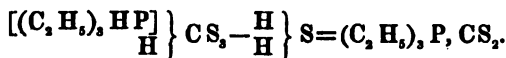
The formation of a compound of this formula by the action of water upon the red crystals is easily understood, if we recollect the

separation, during the principal phase of this process, of disulphide of carbon, and therefore, in consequence of a secondary reaction, of sulphuretted hydrogen which, more especially during the latter stages of the operation, is apt to accumulate to a very considerable extent in the liquid. Three molecules of the red disulphide-of-carbon-compound, and one molecule of sulphuretted hydrogen contain the elements of one molecule of disulphide of carbon, two molecules of sulphide of triethylphosphine and one molecule of the yellow crystals,

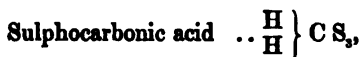


That the formation of the yellow crystals is actually due to the action of sulphuretted hydrogen is unequivocally proved by a synthetical experiment. The red disulphide-of-carbon-compound, when digested at  $100^\circ$  in sealed tubes with a saturated solution of sulphuretted hydrogen, is rapidly converted into the yellow crystals. I have thus succeeded in preparing considerable quantities of this substance, of which, as long as it was only obtained as a secondary, I might almost say accidental, product of the reaction, I had great difficulty in procuring a sufficient quantity for analysis.

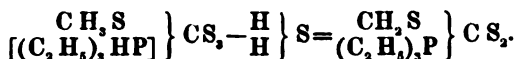
The constitution of the yellow crystals, like that of the red compound from which they originate, is rather obscure. I have already pointed out on a former occasion that the red compound may be viewed as the primary triethylphosphonium-salt of sulphocarbonic acid *minus* one molecule of sulphuretted hydrogen,



If we imagine a sulphocarbonic acid in which one equivalent of hydrogen is replaced by the monatomic radical  $\text{CH}_2\text{S}$  (methylene-mercaptam *minus* the molecular residue of sulphuretted hydrogen),



the yellow crystals may be viewed as the triethylphosphonium-salt of sulphomethylated sulphocarbonic acid *minus* one molecule of sulphuretted hydrogen,



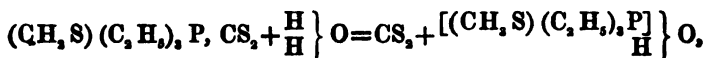
The view which I am taking of the constitution of the yellow crystals is supported by a remarkable transformation which this substance undergoes under the influence of boiling water.

I have already mentioned that this compound cannot be recrystallized from boiling alcohol without loss. The substance is likewise readily soluble in boiling water, but the solution contains no longer the original compound, and no crystals are deposited either on cooling or on evaporation. During ebullition the yellow compound is entirely destroyed, disulphide of carbon being abundantly evolved. The residuary liquid exhibits a faintly alkaline reaction, and yields with acids well crystallized saline compounds. The finest of these salts is the iodide which, being very soluble in boiling and only sparingly soluble in cold water, may easily be obtained in long well-formed needles of perfect purity.

Analysis has proved this substance to contain



This formula represents the iodide of a sulphomethyl-triethylphosphonium, the derivation of which from the yellow compound may be readily traced. One molecule of the latter and one molecule of water contain the elements of one molecule of disulphide of carbon and one molecule of hydrate of sulphomethyl-triethylphosphonium,



which, when treated with acids, yields the corresponding salts.

I have not been able to eliminate the hydrate again from the iodide. When treated with oxide of silver, this compound loses its iodine, but not without its sulphur being likewise attacked, the last traces of which are separated, however, only after protracted ebullition. Considerable quantities of triethylphosphine are evolved during ebullition, and the liquid filtered off from the black precipitate, consisting of sulphide and iodide of silver mixed with metallic silver, contains the hydrates of argento-triethylphosphonium and methyl-triethylphosphonium, together with the sulphates of these bases.

**XV. "Researches on the Phosphorus-Bases."—No. XIV. Action of Triethylphosphine on the Substitution-compounds of Marsh-Gas. By A. W. HOFMANN, LL.D., F.R.S. Received May 24, 1861.**

The formation of diatomic bases by the action of triethylphosphine on dibromide of ethylene, very naturally suggested the idea of studying the deportment of the phosphorus-base with the numerous brominated derivatives of the ethylene-series discovered by Cahours, and lately examined again by A. Lennox. All these compounds act indeed with extraordinary energy upon triethylphosphine; the products, however, of these reactions are far from exhibiting, with regard to the compounds from which they originate, the simplicity and intimacy of relation which theoretical speculation, based on the observation of the action of dibromide of ethylene, might lead us to expect. Most of the brominated substitution-products are apt to lose either bromine or hydrobromic acid, the bromides respectively of triethylphosphine and of triethylphosphonium being formed, together with secondary phosphonium-compounds of subordinate theoretical interest which I have not investigated in detail.

It still remained to study the deportment of the phosphorus-bases with chlorine- and bromine-compounds of simpler constitution. Hoping to obtain more characteristic results in this direction, I was led to submit the compounds which are formed by the action of triethylphosphine on some of the substitution-products of marsh-gas to a more minute examination.

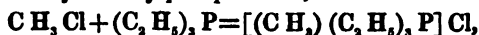
The action of chlorine upon marsh-gas has been the subject of an important investigation by Regnault, who has shown that the gradual substitution of chlorine for hydrogen gives rise to the following series.

Marsh-gas .....	$\text{C H}_4$	Tetrahydride of Carbon.
Monochlorinated marsh-gas ....	$\text{C H}_3 \text{Cl}$	Chloride of Methyl.
Dichlorinated marsh-gas .....	$\text{C H}_2 \text{Cl}_2$	Dichloride of Methylene.
Trichlorinated marsh-gas .....	$\text{C H Cl}_3$	Trichloride of Formyl.
Tetrachlorinated marsh-gas ....	$\text{C Cl}_4$	Tetrachloride of Carbon.

I was anxious to ascertain whether these four chlorinated derivatives, when submitted to the action of triethylphosphine, would fix respectively 1, 2, 3 and 4 molecules of the phosphorus-base, giving

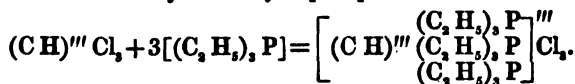
rise to the formation of mono-, di-, tri-, and tetra-phosphonium-compounds, the composition of which was pointed out in advance by theory.

Some of the products which are generated by the action of the chlorinated substitutes of marsh-gas are already known. Chloride of methyl fixes one molecule of triethylphosphine, giving rise to the chloride of methyl-triethylphosphonium,



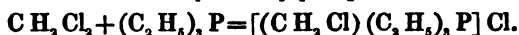
which in combination with dichloride of platinum was examined several years ago by Cahours and myself.

The product which is generated by the action of chloroform on triethylphosphine is likewise known. I have shown that chloroform is capable of fixing three molecules of phosphorus-base, producing the trichloride of formyl-nonethyltriphosphonium,



A short account of this substance has been already communicated to the Royal Society\*.

It remained therefore only to examine the deportment of triethylphosphine with the second, and likewise with the fourth of the chlorinated substitution-products. Dichloride of methylene, the product obtained by the action of chlorine on chloride of methyl, acts with considerable energy on triethylphosphine. Digestion of the mixture for a few hours at a temperature of  $100^\circ$  in sealed tubes, suffices to accomplish the reaction. The products vary considerably, according to the proportion in which the two agents are mixed. One molecule of dichloride of methylene and one molecule of triethylphosphine give rise to the formation of the beautifully crystallized chloride of chloromethyl-triethylphosphonium,



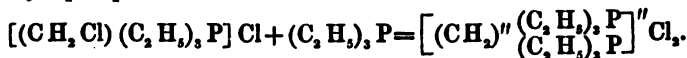
This chloride forms with dichloride of platinum a double salt, beautifully crystallized in needles, rather difficultly soluble in water, which contains



The chloromethylated triethylphosphonium-salt is capable of fixing a second equivalent of triethylphosphine. The crystalline mass

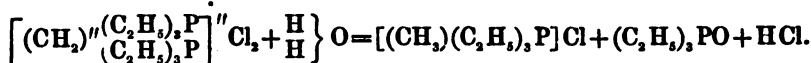
\* Proceedings, vol. x. p. 189.

obtained in this manner is obviously the dichloride of methylene-hex-ethylidiphosphonium,

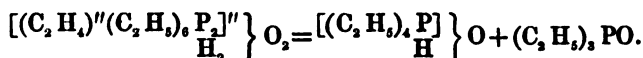


I have not succeeded in seizing the dichloride in a state fit for analysis, since this compound cannot be brought in contact with water without undergoing immediate decomposition; but I have been able to trace the diatomic compound in its products of decomposition. The solution of the dichloride is not precipitated by dichloride of platinum, but, on evaporation, orange-yellow, well-developed octohedra are deposited, which are the platinum-salts of methyl-triethylphosphonium. The mother-liquor of the octohedra furnishes the six-sided tables of the platinum-salt of triethylphosphine.

The following equation represents the transformation of the dichloride of the diphosphonium under the influence of water,



It is thus seen that the diatomic compounds of the methylene-series undergo, even in the form of salts and at the common temperature, a change which in the ethylene-series is observed only with the free bases and under the influence of a high temperature. When hydrate of ethylene-hexethylidiphosphonium is heated, hydrate of tetrethylphosphonium and oxide of triethylphosphine are prominent among the products of decomposition,

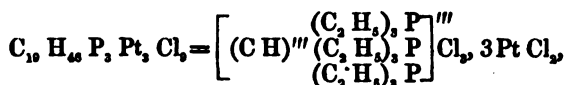


Di-iodide of methylene (obtained by the action of heat upon iodoform) imitates in every respect the deportment of the chlorine-compound with triethylphosphine. The dibromide, on the other hand, which is formed by the action of bromine upon the di-iodide, acts in a perfectly different manner. I propose to examine this reaction in a subsequent communication.

The action of tetrachloride of carbon,  $\text{CCl}_4$ , on triethylphosphine gives rise to changes resembling in many respects the transformations which I have described. On submitting tetrachloride of carbon to the action of triethylphosphine, I was surprised to find that the chloride, which resists with such pertinacity the influence of the

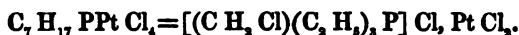
most powerful agents, is attacked with tremendous energy. A drop of triethylphosphine falling into tetrachloride of carbon hisses like water poured upon red-hot iron. It is necessary to operate in vessels filled with carbonic acid gas, to prevent the inflammation of the phosphorus-base, and to accomplish the mixing gradually by using a dropping apparatus. Or the force of the reaction may be diminished by employing the two liquids dissolved in anhydrous ether. Here also the relative proportions in which the two compounds are brought in contact, influence in a measure the result of the reaction; the presence of moisture likewise is apt to change materially the nature of the transformation.

Obtained in presence or in absence of ether, the product of the reaction is a white crystalline exceedingly deliquescent substance. Submitted to the action of water, the crystals dissolve with powerful evolution of heat, giving rise to an intensely acid solution. On adding dichloride of platinum to this liquid, a crystalline precipitate, very difficultly soluble in water, but soluble in boiling concentrated hydrochloric acid, is thrown down, which, on analysis, was found to be the formylated nonethyltriposponium-salt,



which was identified by conversion into the corresponding iodide.

The mother-liquor of the triatomic platinum-salt gave, on evaporation, orange-yellow octohedra containing



The liquid from which this salt had been deposited, furnished, on further evaporation, considerable quantities of oxide of triethylphosphine, which was identified in the form of the beautiful iodide-of-zinc-compound,



described in one of my former Notes.

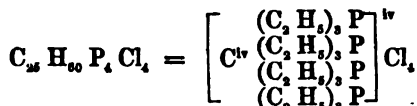
The interpretation of these phenomena presents no difficulties. The existence of the hydrogenated radicals  $(\text{C} \text{H})$  and  $(\text{C} \text{H}_2 \text{Cl})$  in the solution of the products of the action of  $\text{CCl}_4$  on  $(\text{C}_2 \text{H}_5)_3 \text{P}$ , unmistakeably shows that these substances are only secondary products, which owe their origin to the action of water upon the



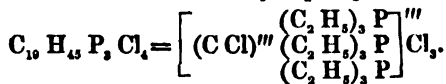
compounds formed directly by the union of triethylphosphine with tetrachloride of carbon. The chemical action of the water is proved, moreover, by the disengagement of heat to which I have alluded, and by the existence of a large quantity of free hydrochloric acid in solution.

I have not succeeded in obtaining the direct product of the action of tetrachloride of carbon on triethylphosphine in a state fit for analysis; the extraordinary deliquescence of the product, and the impossibility of touching it with a solvent without decomposing it, have frustrated my efforts. But here again, exactly as in the case of the diposphonium-compound, the study of the products of decomposition leaves no doubt about the nature of the reaction; it shows that the direct product of the action of tetrachloride of carbon on triethylphosphine is a mixture of two chlorides, viz.—

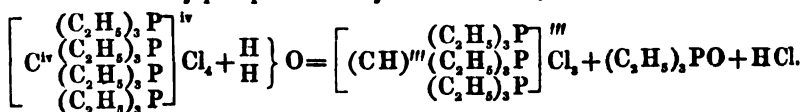
Tetrachloride of carbo-dodeca-ethyltetraphosphonium,



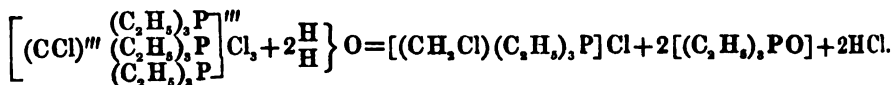
and trichloride of chlorocarbo-nonethyltriposphonium,



The tetraphosphonium-compound, submitted to the action of water, gives rise to the formation of the formylated triphosphonium-salt, oxide of triethylphosphine and hydrochloric acid,



The chlorocarbonated triphosphonium-salt furnishes with water the chloromethylated monophosphonium-compound, oxide of triethylphosphine and hydrochloric acid,

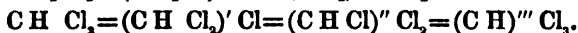
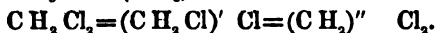
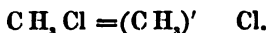


The experiments described in this Note, together with those which I have communicated to the Royal Society on a former occasion, prove that the number of molecules of triethylphosphine which, under

favourable circumstances, can be fixed by the chlorinated derivatives of marsh-gas, is determined by the number of chlorine-equivalents which these substances contain. These bodies need not, however, assimilate the maximum quantities of triethylphosphine represented by these numbers. Thus dichloride of methylene is capable of fixing either one or two molecules of phosphorus-base, the formation of the ephemeral diatomic base being invariably preceded by that of the more stable chloromethylated monophosphonium-compound. Similar phenomena are observed in the action of chloroform and tetrachloride of carbon. The compounds which I have described as resulting from these reactions, are not the only products; by changing the relative proportions of the agents, a number of chlorinated phosphonium-bases are formed so much resembling each other, that I have failed in separating them. Nevertheless, by determining the amount of chlorine and platinum in the mixed platinum-salts obtained from these mixtures, I had no difficulty in recognizing the principle involved in their formation. These substances stand to the final products of the reaction of the triphosphonium and tetraphosphonium-series in relations similar to those which obtain between the chloromethylated monophosphonium and the ephemeral diphosphonium-compound.

The results of these inquiries may be condensed in a few formulæ.

The chlorinated derivatives of marsh-gas exhibit, in their deportment with the phosphorus-bases, the characters of hydrochloric acid. The monochlorinated product has the saturating capacity of one molecule of hydrochloric acid, the dichlorinated product of one or two molecules, the trichlorinated of 1, 2 or 3 molecules, the tetrachlorinated product, lastly, of 1, 2, 3 or 4 molecules:



The action of chloride of methyl on triethylphosphine produces only one salt; that of dichloride of methylene produces two; chloroform is capable of producing three; and tetrachloride of carbon, four salts.

#### *Monochlorinated Marsh-gas.*

Monatomic compound . . . . .  $[(\text{C H}_3) (\text{C}_2 \text{ H}_5)_3 \text{ P}] \text{ Cl}.$

*Dichlorinated Marsh-gas.*

Monatomic salt .....  $[(C H_2 Cl)' (C_2 H_4)_2 P]' Cl.$

Diatomic salt .....  $[(C H_2)'' \begin{matrix} (C_2 H_4)_2 P \\ (C_2 H_4)_2 P \end{matrix}]'' Cl_2.$

*Trichlorinated Marsh-gas.*

Monatomic salt .....  $[(C H Cl_2)' (C_2 H_4)_2 P] Cl.$

Diatomic salt .....  $[(C H Cl)'' \begin{matrix} (C_2 H_4)_2 P \\ (C_2 H_4)_2 P \end{matrix}]'' Cl_2.$

Triatomic salt .....  $[(C H)''' \begin{matrix} (C_2 H_4)_2 P \\ (C_2 H_4)_2 P \\ (C_2 H_4)_2 P \end{matrix}]''' Cl_3.$

*Tetrachlorinated Marsh-gas.*

Monatomic salt .....  $[(C Cl_3)' (C_2 H_4)_2 P] Cl.$

Diatomic salt .....  $[(C Cl_2)'' \begin{matrix} (C_2 H_4)_2 P \\ (C_2 H_4)_2 P \end{matrix}]'' Cl_2.$

Triatomic salt .....  $[(C Cl)''' \begin{matrix} (C_2 H_4)_2 P \\ (C_2 H_4)_2 P \\ (C_2 H_4)_2 P \end{matrix}]''' Cl_3.$

Tetratomic salt .....  $[C^{iv} \begin{matrix} (C_2 H_4)_2 P \\ (C_2 H_4)_2 P \\ (C_2 H_4)_2 P \\ (C_2 H_4)_2 P \end{matrix}]^{iv} Cl_4.$

June 20, 1861.

Major-General SABINE, R.A., Treasurer and Vice-President,  
in the Chair.

In accordance with the Statutes, notice of the ensuing Anniversary Meeting was given from the Chair.

Dr. Heinrich Debus, Mr. Campbell De Morgan, Dr. Thomas A. Hirst, Professor James Clerk Maxwell, Dr. Edmund Alexander Parkes, Professor William Pole, Mr. Philip Lutley Sclater, Professor Henry John Stephen Smith, and Dr. Thomas Thomson, were admitted into the Society.

The following communications were read :—

# I. "On the Lunar Semidiurnal Variation of the Barometer."

By JOHN ALLAN BROWN, Esq., F.R.S., Director of the Trevandrum Observatory. Received May 30, 1861.

The results obtained hitherto for the lunar diurnal variation of atmospheric pressure have been received with some doubt; the range of the variation has been found to be small, and the proximity to the sea of the stations for which the discussions have been made, has given some value to the idea that the variation is due simply to the tidal rise and fall of the sea, the base of the atmosphere. It has even been suggested that the barometer should be incapable of showing an atmospheric tide, because the gravity of the mercury is diminished by the lunar attraction, as well as that of the atmosphere; this suggestion, however, omits all consideration of the integrating means by which a tide is formed.

I have considered the question at two stations in the following manner. Simultaneous observations of two standard barometers of the same construction, with boiled tubes 0.65 inch internal diameter, were made hourly during fifteen months (April 1857 to June 1858) at two observatories; one at Trevandrum 200 feet above the sea-level and three miles distant from the sea, the other on the summit of the Agustier peak of the South Indian Ghats, 6200 feet above the sea, about 22 miles from Trevandrum, 25 miles from the sea on the west, and 40 to 60 miles from the sea on the south and east. The means of all the observations give the following results.

*At Trevandrum.*—The lunar diurnal variation of atmospheric pressure has two nearly equal maxima, occurring almost exactly at the moon's passages of the upper and lower meridian, the minima occurring six hours before and after these epochs.

*At Agustier (6200 feet).*—The law is nearly the same as to epochs, the maximum for the inferior passage occurring, however, about one hour later, and each minimum occurring about one hour nearer the upper passage.

The chief difference in the two results is that between the relative values of the maxima and minima. The oscillation occurring while the moon is below the horizon, or between the meridians of 6<sup>h</sup> and of 18<sup>h</sup>, has the same amount at Trevandrum (height 200 feet) and at Agustier (height 6200 feet); but the oscillation occurring while

the moon is above the horizon, has twice the amount at the lower station which it has at the upper station.

This curious result, which it is difficult to explain by any theory depending simply on the attraction of gravitation, whether acting on the sea or the atmosphere, agrees remarkably with the conclusions for the solar diurnal oscillation obtained from the discussion of hourly observations made simultaneously during a month at five different stations, four of these being on the Agustier mountains, rising gradually from 1200 to 6200 feet, the fifth station being at Trevandrum. From these observations (an abstract of which will be found noticed in the Report of the British Association for 1859, Trans. of Sect., p. 46), it appeared that the solar semidiurnal oscillation between 9 P.M. and 9 A.M. was nearly the same at all the stations; while the day oscillation, 9 A.M. to 9 P.M., diminished with the height, being at 6200 feet little more than half the amount of the oscillation at 200 feet.

These facts might be put into the following general form :—When the heavenly body (sun or moon) is *below the horizon*, the semidiurnal oscillation of the barometer within the tropics has the same value at all heights up to 6200 feet; but when the body is *above the horizon*, the oscillation at 6200 feet has only half the amount of the oscillation at the level of the sea.

II. “On the Law of Disturbance and the Range of the Diurnal Variation of Magnetic Declination near the Magnetic Equator, with reference to the Moon’s Hour-angle.” By JOHN ALLAN BROWN, Esq., F.R.S., Director of the Trevandrum Observatory. Received May 30, 1861.

The discovery by Dr. Lamont of a “decennial” period in the range of the solar diurnal variation of magnetic declination, naturally leads to the question whether a similar law may not exist for the lunar diurnal variation; the question is also of importance in connexion with the theory of the cause of these variations. The following results for the range of the lunar diurnal variation were obtained from the discussion whose conclusions were given in the Proceedings of the Royal Society, vol. x. p. 475.

## Range of Diurnal Variation at Trevandrum, 1854-59.

Year.	Lunar.		Solar.		Ratio. Solar Lunar.
	From variations for groups of 3 months.		From variations for single months.		
1854	....	0.52	....	2.24	4.3
1855	....	0.54	....	2.05	3.6
1856	....	0.41	....	2.01	4.9
1857	....	0.53	....	2.15	4.0
1858	....	0.58	....	2.41	4.2
1859	....	0.66	....	2.64	4.0

It appears from these values that the range of the lunar diurnal period varies from year to year according to the same law (nearly) as the range of the solar diurnal period; and we may conclude that the range of lunar diurnal variation obeys the "decennial" law.

It is well known that the declination needle obeys two diurnal laws due to the solar action; the first of these governs the movement of the north end of the needle from east to west and from west to east, the second determines a superposed movement resulting from the relative amounts of displacement of the needle from its normal position at each hour; the latter is usually termed the diurnal law of disturbance. In order to obtain this law and the values of the disturbance, the quantities  $d_0, d_1, d_2 \dots$  (Proceedings, vol. x. p. 477) are summed for each hour and the means are taken. No similar investigation, as far as I am aware, has as yet been undertaken for the lunar diurnal variation.

I have now arrived at the following conclusions in the discussion for this object of six years' observations of magnetic declination made at the Trevandrum Observatory.

Having arranged the differences containing the lunar effect,

$$\begin{array}{ccccccc}
 d'_0 & d'_1 & d'_2 & \dots & \dots & \dots & \dots \\
 d''_0 & d''_1 & d''_2 & \dots & \dots & \dots & \dots \\
 \vdots & \vdots & \vdots & & & & 
 \end{array}$$

according to the moon's hour-angles from the meridian, and obtained the means,

$$\frac{\sum d'_0}{n-1} = h + \frac{\sum_{23}^0(x')}{n-1}, \quad \frac{\sum d''_1}{n-1} = h_1 + \frac{\sum_{23}^0(x'')}{n-1} \dots,$$

the second differences

$$\begin{array}{l} d'_0 - (h_0), d'_1 - (h_1) \dots\dots \\ d''_0 - (h_0), d''_1 - (h_1) \dots\dots \\ \vdots \qquad \qquad \qquad \vdots \end{array}$$

were taken; these differences, as in the case of the similar investigation for the solar laws, represent the disturbance at each hour-angle of the moon; and when the means of the second differences are taken, the law of the lunar diurnal disturbance will be obtained, if any such law exist.

The small range of the regular lunar diurnal variation, and the derangement produced by the larger disturbances, the effects of which cannot be eliminated in short series of observations, would render identical results from different groups of years improbable, especially if the range of the variation to be determined be small. The following conclusions, however, seem sufficiently confirmed by the discussion of different groups of observations to be accepted.

There is a lunar-diurnal law of disturbance which, from the mean of six years' observation, consists of a

Principal maximum 5 hours before the moon passes the upper meridian.

Principal minimum near the moon's passage of the lower meridian.

Secondary maximum 6 hours after the passage of the upper meridian.

Secondary minimum 1 hour after the passage of the upper meridian.

When the results for each year are compared, the principal maximum and minimum are always shown distinctly; but the values and epochs of the secondary maximum and minimum vary, and that so irregularly, that in the mean for the three years 1857-59 the existence of either is doubtful, only the principal maximum and minimum being well-marked.

In order to determine if the law varied with season, means of groups for quarters of the year were obtained; means also for the groups of half-years, October to March and April to September, were taken. As the results for the quarters forming each half-year agreed with each other, and that for the half-year which contained them, the following conclusions may be accepted as probably true.

*Half-year October to March.*

Principal maximum,  $5\frac{1}{2}$  hours before the moon's passage of the upper meridian.

Principal minimum, 3 hours before the moon's passage of the lower meridian.

Secondary maximum,  $5\frac{1}{2}$  hours after the moon's passage of the upper meridian.

Secondary minimum, 1 hour after the moon's passage of the upper meridian.

*Half-year April to September.*

Maximum,  $4\frac{1}{4}$  hours before the moon's passage of the upper meridian.

Minimum, at the passage of the lower meridian.

In the case of the half-year including the June solstice, the secondary points disappear; this also is the case for each of the quarters forming the half-year. It may be due to this and the preponderance of the disturbance in one half of any given year, that the mean for the whole year shows distinctly or indistinctly the secondary points: the question, however, of the secondary points will be considered hereafter in another discussion including a longer series of observations.

The mean value of the lunar disturbance of magnetic declination for the six years 1854-59 at Trevandrum .. } =  $0^{\circ}.341$

The maximum value at 5 hours before the upper passage =  $0^{\circ}.363$

The minimum value near the inferior passage ..... =  $0^{\circ}.324$

So that the variation of the mean law is only  $0^{\circ}.04$ , or about two seconds and a half.

When we examine the means for the separate years, we find

<i>Mean Lunar Disturbance.</i>					Ratio of range of diurnal variation to 1856.	
			Ratio to 1856.			
1854	....	= $0^{\circ}.320$	....	1.18	....	1.27
1855	....	$0^{\circ}.295$	....	1.08	....	1.31
1856	....	$0^{\circ}.272$	....	1.00	....	1.00
1857	....	$0^{\circ}.326$	....	1.20	....	1.28
1858	....	$0^{\circ}.379$	....	1.39	....	1.41
1859	....	$0^{\circ}.453$	....	1.67	....	1.59

I have not at hand the mean solar disturbances for each year, and cannot therefore compare the two; it will appear, however, from a



comparison with the ranges of the solar diurnal variation (which are known to follow nearly the same law as the law of mean disturbance), that the mean lunar disturbance obeys the same law as the mean solar disturbance, and that the former is probably dependent upon the latter.

III. "Experimental Researches on the Functions of the Vagus and the Cervical Sympathetic Nerves in Man." By AUGUSTUS WALLER, M.D., F.R.S. Received June 6, 1861.

The offices of these two nerves in the animal economy are of such importance as to render it a matter of practical interest to the physician, as well as to the physiologist, to obtain as clear an insight as possible into their functions. It would be impossible to point out any nerves presenting so many questions of daily importance to the practitioner as these, which govern the innervation of the stomach, the heart, the lungs, and the liver.

Hitherto we have been restricted to their investigation in the lower animals, and from the results which have been thus furnished, we have judged of what obtains in man. It is almost needless for me to point out that, although such inferences are generally correct as regards the *motor* phenomena of life, our evidence respecting the *sensory* functions of the nerves is necessarily more uncertain and inconclusive.

My object at present is to describe some results obtained by irritating the vagus and cervical sympathetic in man, in such a way as to ascertain their functions, not only in a state of health, but also their condition in disease, and in various functional derangements to which they are subject.

The means which I adopt for this purpose are the same as that which I have used in the case of the ulnar nerve and of the retina, *i. e.* mechanical irritation by simple pressure on the trunk of the nerve. I generally apply pressure of the fingers at the highest point of the neck behind the ramus of the lower jaw. A moderate amount of pressure applied in this manner is usually sufficient, in the course of about a minute, to produce symptoms which are at once easily recognized as originating from the irritation of the vagus and the sympathetic nerve.

Generally the first sensation experienced is that of want of breath, which is followed by deep and laboured inspirations: this dyspnœa occurs in healthy persons in about a minute or two. While these symptoms are occurring, we observe a considerable disturbance of the heart's action. At first there is increase in the number of its pulsations, with decreased power, as may be easily ascertained by the pulse. To this supervenes a retardation of the heart's action of about 4 to 5 beats a minute of its initial number, at the same time there being decrease of the force of the pulse. These motory affections of the heart are frequently accompanied by slight uneasiness over the precordial region.

At the next stage the heart's action may become so much enfeebled as to produce syncope.

The stomach symptoms which are produced by irritation of the vagus are marked by uneasiness over the stomach, in some cases increasing so as to cause a sensation of nausea.

I have not hitherto observed any motor effects on the stomach, but in the intestines borborygmi occur, as after irritation of the vagus on animals. I am unable at present to state whether any glycosuria is produced.

On the ear corresponding to the side compressed are felt tingling and heat, which often last upwards of half an hour after the removal of pressure.

In only one instance have I been able to detect any influence on the pupil. It at first became dilated, and subsequently contracted after the pressure was removed.

All the above symptoms quickly disappear after the removal of the irritating cause, leaving, however, a degree of lassitude, fatigue, and lowness of spirits, which may last for some hours.

It is easily ascertained that the symptoms above described are not owing to compression of the carotid artery, as they may be produced without obliterating the calibre of the artery; or, *vice versâ*, the course of the blood may be completely interrupted in the artery without producing any of the symptoms enumerated.

On another opportunity I intend to show the applications which this mode of investigating the condition of the vagus presents in a practical point of view, for the diagnosis of various functional derangements or diseases of the vagus.

## IV. "On the Double Tangents of a Curve of the Fourth Order."

By ARTHUR CAYLEY, Esq., F.R.S. Received May 30, 1861.

(Abstract.)

The present memoir is intended to be supplementary to that "On the Double Tangents of a Plane Curve," Phil. Trans. vol. cxlix. (1859) pp. 193-212. I take the opportunity of correcting an error which I have there fallen into, and which is rather a misleading one, viz. the emanants  $U_1, U_2, \dots$  were numerically determined in such manner as to become equal to  $U$  on putting  $(x_1, y_1, z_1)$  equal to  $(x, y, z)$ ; the numerical determination should have been (and in the latter part of the memoir is assumed to be) such as to render  $H_1, H_2, \&c.$  equal to  $H$ , on making the substitution in question; that is, in the place of the formulæ

$$U_1 = \frac{1}{n} (x_1 \partial_x + y_1 \partial_y + z_1 \partial_z) U,$$

$$U_2 = \frac{1}{n(n-1)} (x_1 \partial_x + y_1 \partial_y + z_1 \partial_z)^2 U, \&c.,$$

there ought to have been

$$U_1 = \frac{1}{n-2} (x_1 \partial_x + y_1 \partial_y + z_1 \partial_z)^2 U,$$

$$U_2 = \frac{1}{(n-2)(n-3)} (x_1 \partial_x + y_1 \partial_y + z_1 \partial_z)^3 U, \&c.$$

The points of contact of the double tangents of the curve of the fourth order or quartic  $U=0$ , are given as the intersections of the curve with a curve of the fourteenth order  $\Pi=0$ ; such last-mentioned curve is not absolutely determinate, since instead of  $\Pi=0$ , we may, it is clear, write  $\Pi + MU=0$ , where  $M$  is an arbitrary function of the tenth order. I have in the memoir spoken of Hesse's original form (say  $\Pi_1=0$ ) of the curve of the fourteenth order obtained by him in 1850, and of his transformed form (say  $\Pi_2=0$ ) obtained in 1856. The method in the memoir itself (Mr. Salmon's method) gives, in the case in question of a quartic curve, a third form, say  $\Pi_3=0$ . It appears by the paper "On the Determination of the Points of Contact of Double Tangents to an Algebraic Curve," Quart. Math. Journ. vol. iii. p. 317 (1859), that Mr. Salmon has verified by algebraic transformations the equivalence of the last-mentioned form with those of Hesse, but the process is not given. The object of the present memoir is to demonstrate the equivalence in question,

viz. that of the equation  $\Pi_3=0$  with the one or other of the equations  $\Pi_1=0$ ,  $\Pi_2=0$ , in virtue of the equation  $U=0$ . The transformation depends, 1st, on a theorem used by Hesse for the deduction of his second form  $\Pi_2=0$  from the original form  $\Pi_1=0$ , which theorem is given in his paper "Transformation der Gleichung der Curven 14ten Grades welche eine gegebene Curve 4ten Grades in den Berührungspuncten ihrer Doppeltangenten schneiden," Crelle, t. lii. pp. 97-103 (1856), containing the transformation in question: I prove this theorem in a different and (as it appears to me) a more simple manner; 2nd, on a theorem relating to a cubic curve proved incidentally in my memoir "On the Conic of Five-pointic Contact at any point of a Plane Curve," Phil. Trans. vol. cxlix. (1859), see p. 385, the cubic curve being in the present case any first emanant of the given quartic curve: the demonstration occupies only a single paragraph, and it is here reproduced; and I reproduce also Hesse's demonstration of the equivalence of the two forms  $\Pi_1=0$  and  $\Pi_2=0$ .

V. "Notes on the Atmospheric Lines of the Solar Spectrum, and on certain Spectra of Gases." By Dr. JOHN HALL GLADSTONE, F.R.S. Received May 30, 1861.

In the paper of Sir David Brewster and myself on the lines of the solar spectrum\*, attention was drawn to the following among other phenomena:—

1st. "When the sun descends towards the horizon and shines through a rapidly increasing depth of air, certain lines which before were little if at all visible, became black and well defined, and dark bands appear even in what were formerly the most luminous parts of spectrum." These we termed "atmospheric lines." We did not wish to express by that term anything beyond the fact above mentioned; yet we threw out the idea that these lines may have their origin "in the air that encircles our globe."

2nd. In the case of those artificial flames whose spectra "consist of a series of luminous bands separated by dark spaces . . . these luminous bands sometimes coincide with the dark lines of the solar spectrum."

About the same time Kirchhoff† published his theory that this remarkable coincidence is due to the presence in the atmosphere of

\* Philosophical Transactions, 1860, p. 149.

† Pogg. Ann. cix. pp. 148, 275; cx. p. 187.

the sun of the substances which emit these luminous bands, and their appearance as *dark* lines is because "the spectrum of an incandescent gas becomes reversed, when a source of light of sufficient intensity, giving a continuous spectrum, is placed behind the luminous gas." It also appears that "sodium vapour at a temperature much below that at which it becomes luminous, exerts its absorptive power at exactly the same point of the spectrum as it does at the highest temperatures which we can produce." It is, however, as yet an undetermined question at what temperature below that at which it becomes luminous, a gas ceases to absorb; though, from the close connexion which has been shown to exist between emission and absorption, there can be little doubt that such a limit would be arrived at.

This theory of the origin of Fraunhofer's lines, and the uncertainty just alluded to, suggested the desirableness of comparing the "atmospheric lines" of absorption with the luminous bands which make their appearance in the linear spectra of the light emitted by the different constituents of the atmosphere when sufficiently heated. If all the rays emitted by these different constituents should be found to coincide with the "atmospheric lines," it would show at once the origin of the lines, and prove that gases at the ordinary temperature will absorb rays of the same refrangibility as those they emit when themselves glowing. If, on the contrary, they should be found not to coincide, it would prove by an extreme case (for we operate thus upon many miles of oxygen and nitrogen gases at least) that this connexion between absorption and emission either does not extend to these elements, or is confined within those narrower limits of temperature which theory seems to require. It was also conceivable that the atmospheric lines might coincide with the rays emitted, not by all, but by a portion of the constituents of the atmosphere.

The following data exist for this comparison. Ångström\* has delineated the luminous bands due to the gas when the electric spark is sent through nitrogen, oxygen, carbonic acid, hydrogen, &c. Plücker† has described and measured those bands which appear in the spectra of the same and other gases when they serve as the residuary gas in Geissler's tubes. Under the influence of the electric discharge, the vapour of water, carbonic acid, ammonia, and

\* Pogg. Ann. xciv. p. 141.

† Ibid. cvii. pp. 497, 638.

other compounds are decomposed; but it occurred to me that the oxyhydrogen flame must consist partly, if not entirely, of glowing steam, since it is only when the two elements combine that the heat is evolved; and that similarly the flame of carbonic oxide burning in air must contain the light emitted from glowing carbonic acid. I therefore subjected these two flames to prismatic examination.

The result of the comparison has been that not one of these observations shows any accordance between the luminous bands due to the gas, and the dark lines that make their appearance in the solar spectrum when the sun is shining through a great depth of air. Hydrogen alone is inconclusive. Neither is there any accordance between these luminous bands and the more prominent lines of the ordinary solar spectrum.

This shows that oxygen and nitrogen, and perhaps other gases, though in enormous quantity, do not absorb at the ordinary temperature rays of the same refrangibility as those they emit when heated by the means specified.

It would not be legitimate to infer from this that the atmospheric lines have not their origin in the absorbent power of one or more of the common constituents of the atmosphere. It is well known that some gases when placed before a continuous spectrum produce lines of absorption; among these are bromine and iodine vapour; yet the dark lines caused by these two halogens\* do not coincide with the bright lines into which Plücker found the light of Geissler's tubes containing bromine and iodine to be resolved by the prism; nor have I succeeded in reversing them by bringing these substances into a very hot but little luminous flame. Bright lines were discerned, but in other positions. In connexion with this subject, it may be worth noting that a prismatic examination of the sun's rays passing through three inches of mercury vapour at above 300° C., did not afford the least indication of the reversal of the bright rays that appear when that metal is rendered incandescent.

From the fact that the atmospheric lines do not always present the same appearance when the sun is on the horizon, and that the

\* See "Experiments and Observations on some cases of lines in the prismatic spectrum produced by the passage of light through coloured vapours and gases, and from certain coloured flames," by Prof. W. A. Miller. Phil. Mag. August 1845.

band  $\delta$  has been observed during a shower, and the most prominent lines during a fog, it has been sometimes supposed that the aqueous vapour in the atmosphere is the cause of them. Yet this can scarcely be. They seem not to be due to little vesicles of condensed vapour; for the sun's rays when passing through the edges of a cloud do not exhibit them, unless, of course, near the horizon. And they seem not to be due to gaseous water; for they appear near sunset when the aqueous vapour in the atmosphere is reduced to a minimum by frosty weather, though they are not seen when the sun is higher up in the heavens on a damp warm day. From Sir David Brewster's notes, it appears that February 10, 11, 12, and 13 of the year 1838 were frosty days; yet the lines were well seen:  $\delta$ ,  $\iota$ ,  $\eta$ , C 6, C 15, C 16, and D are specially mentioned. On the 13th, when the thermometer stood at 23° F.,  $\iota$  is said to have been the most prominent.

In the paper referred to at the commencement, we, in common with most of those who worked on the subject before the appearance of Kirchhoff and Bunsen's paper \*, ascribed a bright line coincident with D to other spectra than that of soda. This was no doubt owing to the almost universal trace of that substance.

The electric lights produced between charcoal points by Professor Holmes's magneto-apparatus, and by a galvanic battery with M. Serin's lamp, were found to be identical, when subjected to prismatic analysis. Each exhibited a continuous spectrum, and not those variations of bright and dark which other observers, as well as myself, have noticed in electric lights from charcoal points of but inferior quality. The spectrum extended at both ends beyond that of ordinary direct sunlight, and the only lines which I could discern were bright ones in the violet or lavender region. The following refractive indices were determined for the magneto-electric light; and they are compared with the refractive indices of the nearest principal dark lines of the solar spectrum as determined with the same prism. The relative position of these bright rays and the dark lines in the lavender part of the spectrum, which are only visible in the sun's light under the most favourable circumstances, must not be relied on as accurate, since it was determined by measurement, and not by direct comparison. Possible inaccuracy of adjustment will render the

\* Pogg. Ann. cx. p. 161.

fourth place of decimals quite uncertain. The remarkable extension of the spectrum of this electric light, both at the red and violet ends, is, however, indisputable.

Electric light.	Sunlight.
Limit of red ..... 1·6025 (about)	Line X ..... 1·6038
— — — — —	" A ..... 1·6069
— — — — —	" G ..... 1·6404
Bundle of violet rays ... 1·6436	— — — — —
Faint violet ray ..... 1·6516	" K ..... 1·6513
2nd " " ..... 1·6531 (about)	— — — — —
Bright violet ray ..... 1·6550	" I ..... 1·6548
Faint ray ..... 1·6562 (about)	— — — — —
" " ..... 1·6574 (about)	" L ..... 1·6567
Limit of violet ..... 1·6664 (about)	" N (1st) ..... 1·6642

VI. "On Great Fluctuations of Temperature in the Arctic Winter." By J. J. MURPHY, Esq. Communicated by Professor W. THOMSON. Received June 7, 1861.

It might be expected that the climate of the Arctic Regions during winter, in the absence of the sun, must be almost a dead level of intense cold ; but so far is this from being the case, that there is no other place and time where such great and rapid fluctuations of temperature have been observed.

This phenomenon is thus mentioned in the appendix to Wrangell's account of his expedition to the Siberian coasts of the Polar Sea :—

" Sometimes in the middle of winter a wind from the S.E. by E. causes the temperature to rise suddenly from  $-24^{\circ}$  to  $+25^{\circ}$ , or even  $+32^{\circ}$ : previously to this, the barometer sinks as much as four-tenths of an inch in the course of eight hours. The S.S.E. wind has no particular influence either on the barometer or thermometer."

In "The Search for Sir John Franklin," published in No. 1 of the 'Cornhill Magazine,' occurs the following notice of the same phenomenon. The 'Fox' was beset by vast fields of ice somewhere in Baffin's Bay :—

" December 28. During Divine Service yesterday the wind increased, and towards the afternoon we had a gale from the north-westward, attended with an unusual rise of temperature: today the gale continues, with a warm wind from the N.N.W.

"The Danish settlers at Upernavik, in North Greenland, are at times startled by a similar sudden rise of temperature. During the



depth of winter, when all nature has long been frozen, and the sound of falling water has long been forgotten, rain will fall in torrents; and as rain in such a climate is attended with every discomfort, this is looked upon as a most unwelcome phenomenon. It is called the *warm south-east wind*. Now, if the Greenlanders at Upernavik are astonished at a warm south-east wind, how much rather must the seamen, frozen up in the pack, be astonished at a warm north-west wind! Various theories have been started to account for this phenomenon; but it appears most probable that a rotatory gale passes over the place, and that the rise of temperature is due to the direction from which the whole mass of air may come, viz. from the southward, and not to the direction of the wind at the time\*."

The cause here assigned appears to me quite insufficient: the rise of the thermometer that we have to account for sometimes amounts to 70° or 80°, which is equal to the difference between very warm summer weather and very hard frost in our climate; and it is unexampled, and I think inconceivable, that any motion of a mass of air from warmer latitudes should produce so great an effect on the temperature; certainly the cyclones that come from the West Indian Seas and pass over our islands have no effect in the slightest degree approaching to it.

What I regard as the true cause of the phenomenon is suggested, though not distinctly pointed out, in Dr. Kane's Narrative, from which I will make a few extracts:—

"January 29. A dark water sky extended in a wedge from Littleton to a point north of the Cape. Everywhere else the firmament was obscured by mist. The height of the barometer continued as we left it at the brig, and our own sensations of warmth convinced us that we were about to have a snow-storm. \* \* \* We were barely housed before the storm broke upon us. Here, completely excluded from the knowledge of things without, we passed many miserable hours. We could keep no note of time, and, except by the whirring of the drift against the roof of our kennel, had no information of the state of the weather. \* \* \* We then turned in to sleep again, no longer heedful of the storm, for it had buried us deep in with the snow. But in the meantime, although the storm continued, the temperatures underwent an extraordinary change. I was awakened by the dropping of

\* The writer in the 'Cornhill Magazine' prints this second paragraph in inverted commas, but does not state whence he has quoted it.

water from the roof above me ; and upon turning back my sleeping bag, found it saturated by the melting of its previously condensed hoar-frost. My eider-down was like a wet swab. I afterwards found that the phenomenon of the warm south-east had come unexpectedly upon us. The thermometers at the brig indicated  $+26^{\circ}$ , and, closer as we were to the water, the weather was probably above the freezing-point. When we left the brig—how long before it was we did not know—the temperature was  $-44^{\circ}$ . It had risen at least seventy degrees. \* \* \* In the morning—that is to say, when the combined light of the noon-day dawn and the circumpolar moon permitted our escape—I found, by comparing the time as indicated by the Great Bear with the increased altitude of the moon, that we had been pent up nearly two days.”

It appears from these extracts, that although Dr. Kane did not see open water, he was made aware of its neighbourhood by the infallible sign of a “Water Sky.” A rise of temperature to a few degrees above frost would be quite insufficient to produce open water by melting through the fields of ice in forty-eight hours ; but, on the other hand, the breaking up of the fields of ice by a storm is an adequate cause for a great rise of temperature ; for the water immediately below the ice is at the temperature of sea-water at its freezing-point, which is  $+28^{\circ}$  ; so that when a storm comes and breaks up the ice, the water comes into contact with air  $70^{\circ}$  or  $80^{\circ}$  colder, and warms the air.

There is no doubt of the power of a storm to break up the ice. Sir James Ross speaks of “the almost magical power of the sea in breaking up land-ice or extensive floes of from twenty to thirty feet thick, which have, in a few minutes after the swell reached them, been broken up into small fragments by the power of the waves.” The theory that these sudden rises of temperature are caused by storms breaking up the ice and exposing the comparatively warm water below, also harmonizes with the fact that the warm winds, as mentioned by the officer of the ‘Fox,’ in different parts of Baffin’s Bay come from different points of the compass ; while on the same coast they come from the same point. Thus Wrangell, as quoted above, mentions that in the part of the Siberian coast which he explored, a S.E. by E. wind sometimes raises the thermometer upwards of fifty degrees, while a S.S.E. wind has no effect on the temperature at all. This proves that the rise of temperature cannot be due to the transport of a mass

of warm air ; but it may be easily accounted for by supposing that the form of the coast enables the warmth-producing wind to act at a special advantage in breaking up or driving away the ice, and liberating the heat of the waters.

These extraordinary fluctuations of temperature appear to be common to the whole of the Arctic regions. Sir John Richardson, in his recent work on the Polar regions, states that "in Arctic America the phenomenon of warm winds (*teplot weter* of Wrangell) also occurs, and makes the month in which they happen, whether December, January, or February, warmer than the other two. The same warm wind was probably the cause of the rain which the Russian sailors observed in Spitzbergen in the month of January."

Rain implies a temperature several degrees above  $+28^{\circ}$ , which is the temperature of the stratum of sea-water immediately below the ice. But we know that in the Polar regions the temperature of the sea increases in descending, until a stratum is reached of the invariable temperature of  $+39^{\circ}$ ; and we may suppose that in these storms the warmer water of the deeper strata is brought to the surface, and warms the air sufficiently to admit of rain. We know that powerful winds are able to produce temporary local currents, and it is easy to see that such a current, when produced in a limited space free of ice, will give rise to this kind of *vertical circulation*, or interchange between strata of different depths.

Such storms as these must be eminently favourable to the production of rain ; for the air that becomes warmed by contact with the comparatively warm water will, of course, take up watery vapour, and when it comes into contact with other masses of air that retain their usual intense cold, the vapour will be rapidly condensed ; so that we cannot wonder at heavy rains being a general concomitant of these storms.

Wrangell, in the passages I have quoted, says the warm wind in Siberia is preceded by a fall of the barometer. Dr. Kane, on the contrary, noticed a rise before the storm above described ; it stood at "the extraordinary height of 30.85." I cannot suggest any explanation of these facts.

I believe I have now stated the true cause of what is certainly a very remarkable phenomenon—fluctuations of temperature of enormous magnitude, occurring in a very short time, and in the absence of the sun.

VII. "On the Measurement of Electric Resistance." By Professor W. THOMSON, F.R.S. Received June 6, 1861.

Part I. *New Electrodynamical Balance for resistances of short bars or wires.*

In measuring the resistances of short lengths of wire by Wheatstone's Balance \*, I have often experienced considerable difficulty in consequence of the resistances presented by the contacts between the ends of the several connected branches or arcs. This difficulty may generally be overcome by soldering or amalgamating the contacts, when allowable; but even with soldered connexions there is some uncertainty relating to the dimensions of the solder itself, when the wires tested are very short. When soldering was not admissible, I have avoided being led into error, by repeating the experiment several times with slightly varied connexions; but I have in consequence sometimes altogether failed to obtain results by either Wheatstone's or any other method hitherto practised, as for instance in attempting to measure the electric resistances of a number of metallic bars each 6 millimetres long and 1 millim. square section, which were put into my hands by Mr. Calvert of Manchester, being those of which he and Mr. Johnstone determined the relative thermal conductivities in their investigation published in the Transactions of the Royal Society for March 1858. I have thus been compelled to plan a new method for measuring electric resistances in which no sensible error can be produced by uncertainty of the connexions, even though made with no extraordinary care.

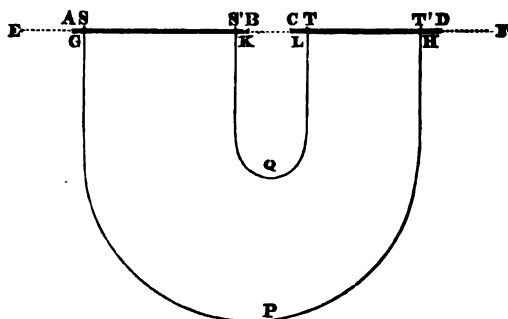
Let AB and CD be the standard and the tested conductors respectively. Let the actual standard of resistance be the resistance of the portion of AB between marks † S, S' on it, and let it be required to find a portion TT' of CD which has a resistance either equal, or bearing a stated ratio, to that standard.

Join BC either by direct metallic contact between them, or by

\* I have given this name to the beautiful arrangement first invented by Professor Wheatstone, and called by himself a "differential resistance measurer." It is frequently called "Wheatstone's Bridge," especially by German writers. It is sometimes also, but most falsely, called "Wheatstone's Parallelogram."

† On the same principle as the "mètre à traits" instead of the "mètre à bouts" for a standard of length.

any ordinarily good metallic connexion with binding screws or otherwise; and join the two electrodes of a galvanic element to their other ends, A, D. Let GPH and KQL be two auxiliary conductors, which, to avoid circumlocutions, I shall call the primary and the



secondary testing-conductors respectively, with their ends applied to the marked points S, T', S', T. Let P and Q be points in these conductors to which the electrodes of the galvanometer are to be applied.

It is easily seen, and will be demonstrated below, that if the resistances of the testing-conductors be similarly divided in Q and P, and if their ends be in perfect conducting communication with the marked points of the main line to which they are applied, the condition that the galvanometer indication may be zero is that the ratio of the resistances of the standard and tested conductors must be the same as that in which the auxiliary conductors are each divided. Further, it is clear that by making the testing-conductors of incomparably greater resistances than any that can exist in the connexions at S, S', T, T', which can easily be done if these connexions are moderately good, the error arising from such imperfections as they must present may be made as small as is required\*. To de-

\* This method may be readily applied to Siemens's mercury standards (see Phil. Mag. Jan. 1861, or Poggendorff's 'Annalen,' 1860, No. 5), by introducing platinum wires through holes in the glass tube near its ends, as electrodes for the testing-conductors, and wires or plates of platinum at the ends, as electrodes for one pole of the battery and for connexion with the conductor to be compared with it, respectively. It will then not be the whole line of mercury from end to end, but the portion of it between the two platinum wires first mentioned, that will be the actual standard. The objection against the use of mercury as a standard of resistance, urged by Matthiessen, that the amalgamated copper

monstrate the above and to form an accurate idea of the operation of this method, it is necessary to investigate the difference of potentials (electromotive force) produced between Q and P, when a stated difference of potentials, E, is maintained between S and T'.

Let SS', TT' denote the resistances between the marks, on the standard and tested conductors respectively. Let GPH, GP, PH, KQL, KQ, QL denote the resistances of the testing-conductors and their parts according to the diagram, implying that

$$GPH = GP + PH, \text{ and } KQL = KQ + QL.$$

Let SG, HT', S'K, LT be the resistances in the connexions at the marks; let

$$SG + GPH + HT' \text{ be denoted by } SPT',$$

and

$$S'K + KQL + LT \quad ,, \quad ,, \quad S'QT';$$

and let S'BC T denote the resistance between S' and T composed of the resistance in the connexion and the resistances in the portions of

electrodes which Siemens found necessary to give very perfect *end* connexions must render the mercury impure and increase its resistance sensibly after a time, is thus completely removed. It must be shown, however, that different specimens of commercial mercury, dealt with in the manner prescribed by Siemens, to remove impurities, shall always be found to have equal specific resistances, before his proposal to produce independent standards by filling gauged tubes with mercury can be admitted as valid. But the *transportation and comparison of actual standards* between different experimenters in different places is, and probably must always be, the only way to obtain *the most accurate possible common system of measurement*: and when a proper mutual understanding between electricians and national scientific academies, in all parts of the world, has been arrived at, as it is to be hoped it may be soon, through the assistance of the British Association and Royal Society if necessary, the use of definite metallic standards, whether the liquid mercury as proposed by Siemens, on the one hand, or the solid wire, alloy of gold and silver, on the other hand, proposed by Matthiessen (Phil. Mag. Feb. 1861), would be essential only in the event of all existing standards being destroyed.

Weber's absolute system is often referred to as if its object were merely to fix standards of resistance, and the difficulty and expense of applying it independently have been objected to as fatal to its general adoption. In reality its great value consists in the dynamic conditions which it fulfils, with relation to electro-magnetic induction, and to the mechanical theories of heat and of electro-chemical action. But it most probably will also be much more accurate than any definite metallic convention, for the re-establishment of a common metrical system, in case of the destruction of all existing standards.

the main conductors from the marks  $S'$  and  $T$  to their ends. Lastly, let  $R$  denote the resistance in the double channel  $\left\{ \frac{S'BCT}{S'QT} \right\}$  between  $S'$  and  $T$ . By the well-known principles of electric conduction, we have

$$R = \frac{1}{\frac{1}{S'BCT} + \frac{1}{S'QT}} \dots \dots \dots (1)$$

for the resistance in the double arc between  $S'$  and  $T$ . Then, by addition, we have

$$SS' + R + TT',$$

for the resistance from  $S$  to  $T'$  by the channel  $SS' \left\{ \frac{S'BCT}{S'QT} \right\} TT'$ .

This whole resistance is divided, by  $Q$  and its equipotential point in the direct channel  $S'BCT$ , into the parts

$$SS' + \frac{S'Q}{S'QT} \cdot R, \text{ and } \frac{QT}{S'QT} \cdot R + TT'.$$

Hence if, for simplicity, we suppose the potential at  $S$  to be 0, and at  $T'$  to be  $E$ , and if we denote by  $q$  the potential at  $Q$ , we have

$$q = E \frac{SS' + \frac{S'Q}{S'QT} \cdot R}{SS' + R + TT'} \dots \dots \dots (2)$$

Again, since  $P$  divides the resistance between  $S$  and  $T'$ , along the channel  $SPT'$ , into the parts  $SP$  and  $PT'$ , we have

$$p = E \frac{SP}{TT'} \dots \dots \dots (3)$$

if we denote by  $p$  the potential at  $P$ . Hence

$$q - p = E \frac{SS' + \frac{S'Q}{S'QT} \cdot R - \frac{SP}{SPT'} (SS' + R + TT')}{SS' + R + TT'};$$

or, since  $1 - \frac{SP}{SPT'} = \frac{PT'}{SPT'}$ ,

$$q - p = E \frac{\frac{PT'}{SPT'} \cdot SS' - \frac{SP}{SPT'} \cdot TT' + R \left( \frac{S'Q}{S'QT} - \frac{SP}{SPT'} \right)}{SS' + R + TT'} \dots \dots (4)$$

Now let us suppose that, by varying one or more of the component

arcs in the balance-circuit, we reduce the galvanometer indication to zero, that is to say, make  $q-p=0$ . We shall have by equating the numerator of the preceding expression to zero, and resolving for  $TT'$ ,

$$TT' = \frac{PT'}{SP} \cdot SS' + R \left( \frac{SPT'}{S'QT} \cdot \frac{S'Q}{SP} - 1 \right). \quad (5)$$

To interpret this expression, it may be remarked that if the second term vanishes, that is to say, if

$$\left. \begin{aligned} R \left( \frac{SPT'}{S'QT} \cdot \frac{S'Q}{SP} - 1 \right) &= 0 \\ TT' &= \frac{PT'}{SP} \cdot SS' \end{aligned} \right\}; \quad (6)$$

we have

and this is the condition aimed at in the arrangement. Now the connexions at S and T' must be made so good that the resistance SG in the first is inappreciable in comparison with GP, and the resistance HT', in the second, inappreciable in comparison with PH; so that we may have

$$\frac{PT'}{SP} \approx \frac{PH}{GP},$$

where  $\approx$  denotes an equality not perfect, but having no appreciable error: and hence

$$TT' \approx \frac{PH}{GP} \cdot SS'.$$

The condition

$$R \left( \frac{SPT'}{S'QT} \cdot \frac{S'Q}{SP} - 1 \right) \approx 0$$

is to be secured by one or other of two ways or by both combined; that is, by making

$$R \approx 0 \quad (a)$$

or

$$\frac{SPT'}{S'QT} \cdot \frac{S'Q}{SP} \approx 1, \quad (b)$$

or each as nearly as possible. If the connexion BC were quite perfect and the marks S' and T were at the very ends of the conductors, the condition (a) would be fulfilled and there would be no necessity for the condition (b). We should then have a perfect Wheatstone balance,—the secondary testing-conductor  $\left\{ \begin{smallmatrix} K \\ L \end{smallmatrix} \right\} Q$  becoming merely a part of the galvanometer electrode. Hence whenever the re-



sistance  $S'T$  can be made absolutely insensible, Wheatstone's balance leaves nothing to desire, provided the ends of the testing-conductor are applied to marked points on the standard and tested conductors, and the battery electrodes to their outer ends, or to points of them between their outer ends and those marked points. When, however, as very frequently is the case,  $S'T$  may be made small but not absolutely insensible in comparison with the resistances of the standard and tested conductors, the addition of the "secondary testing-conductor" becomes valuable, even if it be only arranged to give a rough approximation to the condition  $\frac{SPT'}{S'QT} \cdot \frac{S'Q}{SP} = 1$  \*, since it will reduce the error to the fraction  $\frac{SPT'}{S'QT} \cdot \frac{S'Q}{SP} - 1$ , of the small resistance  $R \dagger$ . But further, when, as in experiments on short thick bars like those of Mr. Calvert,  $S'T$  cannot by any management be got to be small in comparison with  $TT'$ , the use of the secondary testing-conductor becomes essential, and the most accurate possible fulfilment of the condition

$$\frac{SPT'}{S'QT} \cdot \frac{S'Q}{SP} = 1$$

must be aimed at. This is to be done by dividing the secondary testing-conductor at  $Q$ , in very exactly the same ratio as the primary at  $P$ , and taking care that the resistances in the connexions  $S'K$ ,  $LT$  are very small in comparison with  $KQ$  and  $QL$ .

## Part II. *Suggestions for carrying out these principles in practice.*

When high accuracy is not required, the two "testing-conductors" may be made of wires stretched straight in parallel lines, and the connexions for the galvanometer electrodes may be applied to them by means of a slide on a graduated scale—as in one of the common forms of Wheatstone's balance, with sliding contact on single testing-conductor. This form is very objectionable, however, whether for Wheatstone's balance or the method I now propose: (1) because

\* This of course is equivalent to  $SPT' : SP :: S'QT : S'Q$ , and means that the secondary conductor is to be divided by one galvanometer electrode in the same proportion as the primary is divided by the other.

† In such cases  $R$  will, according to equation (1) above, be nearly equal to  $S'BOT$ , but somewhat less.

it is impossible to secure that the different parts of each testing-conductor shall be accurately at the same temperature ; (2) because the resistances at the ends of the fine stretched wire or wires are always sensible in comparison with the smallest measured differences produced by the slide ; (3) because the stretched wire itself is never of absolutely equal gauge throughout, and, even if sensibly so when first put into the instrument, soon ceases to be so in consequence of the friction of the sliding contact which it experiences in use\* ; (4) because, in even the hastiest experiments, provided a rationally planned galvanometer is used, a far higher proportional degree of accuracy is easily attained in measuring electrical resistances against a standard of resistance than can be at all attained, without very extraordinary precautions and the assistance of a microscope, in measuring lengths under a yard or two against a standard of length.

When the highest accuracy is required, I always use for primary testing-conductor the bisected conductor which I described to the British Association at its Glasgow meeting in 1855. This consists of a fine, very perfectly insulated wire, doubled on itself and wound on a bobbin, with very stout terminals soldered to its ends, and an electrode soldered to its middle, for joining to the galvanometer electrode. The two terminal and the middle electrodes thus attached to the testing-conductor, I have generally hitherto made flexible, either of thick wire, or strand of wires like the conductor of a submarine cable ; but, for many applications, it is more convenient to make them solid metal blocks, with binding screws, insulated rigidly upon the bobbin which bears the conductor. The two halves into which the conductor is doubled must be very accurately equalized as to electric resistance when they are wound on the bobbin, and before the terminals are finally attached. This I find can be done with great accuracy ; and when, after the terminals are soldered on, the electric bisection is once found perfect, it seems to remain so, without sensible change, for years. The close juxtaposition of the two branches of the testing-conductor on this plan ensures an almost absolute equality of temperature between them in all circumstances,

\* This defect I have remedied by frequently putting in a new wire for testing-conductor in working with a sliding-scale Wheatstone's balance.

and thus renders easy a degree of accuracy in the measurement of resistances quite unattainable with any other form of Wheatstone's balance. In the new method which I now propose for low resistances, I make the secondary conductor on exactly the same plan, and generally of about the same dimensions, as the primary. The bisected testing-conductors are only available when the resistances of the standard and of the tested conductor can be made equal ; and with them the method which has been described above seems to be the most accurate possible for testing a perfect equality of resistance between two conductors.

The same plan of testing-conductors seems still the best, even when testing by equality cannot be practised,—with only this difference, that the two branches of each testing-conductor, instead of being made of equal resistance, must be adjusted to bear to one another very exactly the ratio which the tested resistance is to bear to the standard. By proper care, to prevent the bobbin of either testing-conductor from getting any non-uniform distribution of temperature, great accuracy may still be secured ; but it is scarcely possible to maintain so very close an agreement of temperature, and therefore so constant a ratio of resistances, as when the two branches are equal lengths of one wire coiled side by side.

The use of this plan of conductors divided in a fixed ratio, whether for the single testing-conductor in Wheatstone's balance, or for the primary and secondary testing-conductors in the new method now proposed, requires that either the standard or the tested conductor can be varied so as to adjust the resistance of one to bear precisely that ratio to the resistance of the other. In certain cases this may be done advantageously by shifting one or other of the contacts  $S, S', T, T'$  along the standard or the tested conductor, as the case may be. If, for instance,  $T$  or  $T'$  can be shifted conveniently, the object of the measurement may be to find by trial on the tested conductor a portion  $TT'$  from mark to mark, of which the resistance bears a stated ratio to the fixed standard  $SS'$  from mark to mark. But by far the easiest working, and in most cases the most accurate also, is to be done by means of a well-arranged series of standards with terminals adapted for combining them in such a manner as to give to a minute degree of accuracy whatever resistance may be required. In a future communication on standards of electric resistance, I

intend to describe plans for attaining this object through a wide range of magnitude (resistances from  $10^5$  to  $10^{13}$  British absolute units of feet per second on Weber's invaluable system). In the mean time I shall merely say that I have formed a plan which I expect will prove very advantageous for low resistances, and which consists in combining the standards, whichever of them are required, in multiple arc (or "parallel" arcs, according to an expression sometimes used), so as to add their *conducting powers*\*,—instead of in series, as in all arrangements of resistance coils hitherto used, by which the *resistances* of the component standards are added.

### Part III. *General Remarks on Testing by Electro-dynamic Balance.*

I shall conclude by remarking that the sensibility of the method which has been explained, as well as of Wheatstone's balance, is limited solely by the heating effect of the current used for testing. To estimate the amount of this heating effect, let  $e$  and  $f$  be the parts of the whole electromotive force,  $E$ , which act in the standard  $SS'$ , and tested conductor  $TT'$  respectively; so that, in accordance with the notation used above, we have

$$\left. \begin{aligned} e &= E \frac{SS'}{SS' + R + TT'} \\ f &= E \frac{TT'}{SS' + R + TT'} \end{aligned} \right\} \dots \dots \dots (7)$$

of its substance. Following Weber, I define the resistance of a bar or wire one foot long, and weighing one grain, its *specific resistance*. It is much to be desired that the *weight-measure*, rather than the *diameter* or the *volume-measure*, should be generally adopted for accurately specifying the gauge of wires used as electric conductors.

With reference to either  $SS'$  or  $TT'$  (the first, for instance), let us use the following notation:—

- $l$  its length in feet;
- $w$  its mass per foot in grains;
- $s$  the specific heat of its substance;
- $\sigma$  the specific resistance of its substance.

\* The reciprocal of the resistance of a "conductor" or "arc" I call its *conducting power*. The conducting power of a bar or wire of any substance one foot long and weighing one grain, I call the *specific conductivity of its substance*.

Thus, since we have taken  $SS'$  to denote its actual resistance, we have

$$SS' = \frac{\sigma l}{w}.$$

Now, Weber's system of absolute measurement for electromotive forces and for resistances being followed, I have shown\* that *the mechanical value of the heat generated per unit of time in any fixed conductor of uniform metallic substance is equal to the square of the electromotive force between its extremities, divided by its resistance.* This in the present case is equal to

$$\frac{e^2 w}{l \sigma};$$

and if  $J$  denote Joule's mechanical equivalent of the thermal unit, we therefore have

$$\frac{e^2 w}{J l \sigma}$$

for the rate per second at which heat is generated in  $SS'$ . This will at first go entirely to raise its temperature†. Now  $wl$  is its mass in grains, and therefore  $wls$  is its whole thermal capacity; and if we divide the preceding expression by this, we find

$$\frac{e^2}{J P s \sigma}$$

for the rate per second at which it commences to rise in temperature at the instant when the battery is applied. If we call  $\frac{e}{l}$  the electromotive force per foot, we may enunciate the result thus:

*The rate at which a linear conductor of uniform metallic substance commences rising in temperature at the instant when an electric current commences passing through it, is equal to the square of the electromotive force per unit of length divided by the continued product of Joule's equivalent into the specific heat of the substance, into the specific resistance of the substance.*

Let us suppose, for example, that the conductor in question is

\* In a paper "On the Mechanical Theory of Electrolysis," Philosophical Magazine, Dec. 1851.

† As soon as it has risen sensibly in temperature it will begin to give out heat by conduction, or by conduction and radiation, to the surrounding matter; and the rate at which it will go on rising in temperature will be the rate expressed by the formula in the text (with the true specific resistance, &c., for each temperature), diminished by the rate of loss to the surrounding matter.

copper of best electric conductivity. Its specific resistance will be about  $7 \times 10^6$ , and its specific heat about  $\cdot 1$ . The value we must use for Joule's equivalent will be 32.2 times the number 1390, which Joule found for the mechanical value in foot-grains of the thermal unit Centigrade, since the absolute unit of force, being that force which acting on a grain of matter during a second of time generates 1 foot per second of velocity, is  $\frac{1}{32.2}$  of the weight of a grain in middle latitudes of Great Britain. Thus we find

$$J = 44758.$$

Hence the expression for the rate in degrees Cent. per second, at which the temperature begins rising in a copper conductor, is

$$\frac{\left(\frac{e}{l}\right)^2}{313 \times 10^9}.$$

I have found the electromotive force of a single cell of Daniell's to be about  $2.3 \times 10^6$  British absolute units\* ; and if we suppose  $\frac{1}{\pi}$  of this to go to each foot of the conductor in question, we shall have

$$\left(\frac{e}{l}\right) = \frac{2.3^2 \times 10^{12}}{\pi^2} = \frac{5.29 \times 10^{12}}{\pi^2};$$

and therefore the expression for the rate of heating becomes

$$1.69 \times \frac{100}{\pi^2}.$$

Now, by using a sufficiently large single cell, we may make the electromotive force,  $E$ , between  $S$  and  $T'$ , be as little short as we please of the whole electromotive force of the cell. We might then, in testing by equality, with a standard and a tested conductor each three inches or so long, and using a single cell, have nearly as much as half the electromotive force of one cell acting per quarter foot of these conductors, or two cells per foot. Hence if either is of best conductive copper, its temperature would commence rising at the rate of  $4 \times 169^\circ$  or  $676^\circ$  Cent. per second. It would be almost impossible to work with so high a heating effect as this. But if we use only  $\frac{1}{10}$ th of the supposed electromotive force, that is to say  $\frac{1}{10}$ th of a cell per foot of the copper conductor, the rate of heating will be reduced to  $\frac{1}{10}$ th, that is to say, will be  $6^\circ.76$  per second. By using

\* Proceedings of the Royal Society, February 1860.

only very brief battery applications, it would be possible to work with so high a rate of heating as that, without having the results much vitiated by it. But  $\frac{1}{80}$  of a cell per foot will give only  $\cdot 0676^\circ$  of heating effect per second, and will be quite a sufficient battery power to use in most cases. In the case we have supposed, for instance, of conductors only three inches long, the electromotive force on each would then be about  $\frac{1}{300}$  of the electromotive force of the cell. What we denoted above by  $e$  and  $f$  in equations (7) would therefore each have this value. Hence, by equation (4), we see that the effect of a difference of  $\frac{1}{10000}$  between  $SS'$  and  $TT'$  would be to give  $q-p$  the value  $\frac{1}{4000000}$  of the electromotive force of a single cell. Now one of the light mirror\* galvanometers, which I commonly use, reflecting the image of a gas or paraffine lamp to a scale 25 inches distant, would, if made with a coil of 50 yards of copper wire of moderate quality, weighing 5 grains per foot, give a deflection of half a division of  $\frac{1}{40}$  of an inch on this scale, with an electromotive force of  $\frac{1}{4000000}$  of a single cell†. Hence by using such a galvanometer, and primary and secondary conductors of sufficient resistances to fulfil the condition of doing away with sensible error from imperfect connexions in the manner explained above, but yet of resistances either less than or not many times greater than the resistance of the galvanometer coil, it is easy to test to  $\frac{1}{10000}$  the resistance of a copper wire or bar not more than 3 inches long. The

\* The mirror is a circle of thin "microscope glass" about three-eighths of an inch in diameter, silvered in the ordinary manner; and a small piece of flat file steel of equal length, attached to its back by lac varnish, constitutes the "needle" of the galvanometer. The whole weight of mirror and needle amounts to from 1 to  $1\frac{1}{2}$  grain. It is suspended inside the galvanometer coil by single silk fibre about  $\frac{1}{4}$  inch long. It is necessary to try many mirrors thus prepared, each with its magnet attached, before one is found giving a good enough image. I am much indebted to Mr. White, optician, Glasgow, for the skill and patience which he has applied to the very troublesome processes involved.

† In this state of sensibility the needle is under Glasgow horizontal magnetic force of the earth alone; and, with its mirror, it makes a vibration one way in about  $\cdot 7$  of a second. In many uses of my form of mirror galvanometer, both for telegraphic and for experimental purposes, I find it convenient to make its indications still more rapid, though, of course, less sensitive, by increasing the directive force by means of fixed steel magnets. On the other hand, I use fixed steel magnets to diminish the earth's directing force and make the needle more sensitive, when very high sensibility is wanted; but this would be inconvenient for the application described in the text, because effects of thermo-electric action would be made too prominent.

current we have found to be sufficient for this object would only produce a heating effect of  $\cdot 14^{\circ}$  in two seconds, which, with good apparatus, is more than enough of time, as I shall show presently. The influence of this heating effect may be regarded as nearly insensible, since even as much as  $\cdot 2^{\circ}$  only alters the resistance of copper by about  $\frac{3}{4000}$ .

In all measurements of electric resistance, whatever degree of galvanic power is used, a spring "make and break" key\* ought to be placed in one of the battery electrodes, so that the current may never flow except as long as the operator wills to keep it flowing, and presses the key. I introduce a second similar spring key in one of the galvanometer electrodes (that is between either Q or P and the galvanometer coil), so arranged that the pressure of the operator's finger on a little block of vulcanite attached to either spring shall first make the contact of the first spring (completing the battery circuit), and when pushed a little further, shall make the contact of the second spring and complete the galvanometer circuit. The test for the balance of resistances will then be that not the slightest motion of the needle is observable as a consequence of this action on the part of the operator. The sensibility of the arrangement is doubled by a convenient reverser in the galvanometer circuit, by which the current, if any, may be reversed easily by the operator while keeping the two connexions made by full pressure on the double spring key just described. Another convenient reverser should be introduced into the battery circuit, to eliminate effects of thermo-electric action if sensible.

It may often happen, unless the galvanometer is at an inconveniently great distance from the conductors tested, that its needle will be directly affected to a sensible extent by the main testing-current; but with the arrangement I have proposed the observer tests whether or not this is the case by pressing the double spring-key to only its middle position (battery contact alone made), and watching whether or not the needle moves perceptibly. If it does not move perceptibly, he has nothing more to do than immediately to press the double key home, to test the balance of resistances. If the needle does move when the key is pressed to its middle position, he may, when in

\* Morse's original telegraph key, which instrument-makers have "improved" into the in every respect worse form in which it is now commonly made—a massive contact-lever urged by a spring.



other respects allowable, keep the current flowing by holding the key in its middle position till the needle comes to rest, or at least till it shows the point towards which its oscillations converge, and then press home to test the balance of resistances. When the very highest accuracy is aimed at, or when, for any reason (as, for instance, extreme shortness in the standard or tested conductor), only the shortest possible duration of current is allowable, the position of the galvanometer, with reference to the battery and the other portions of circuit, must be so arranged that its needle may show no sensible deflection when the key is pressed to the middle position. Ignorant or inadvertent operators are probably often led into considerable mistakes in their measurements of resistance by confounding deflections due to direct electro-magnetic influence of battery, battery electrodes, or standard, tested, or testing-conductors, on the needle of the galvanometer, with the proper influence of a current through its own coil,—a confusion which can only be resolved by making or breaking the galvanometer circuit while the battery circuit is kept made, for which there is no provision in the ordinary plans of Wheatstone's balance. We may, however, suppose that most experimenters will be sufficiently upon their guard against error from such a source. But there is another and a much more important advantage in the double-break arrangement which I now propose. Electro-magnetic inductions will generally be sensible\* in some or in all of the different branches of the compound circuit, and cannot, except in very special cases, be exactly balanced as regards electromotive force between P and Q with the arrangement which makes an exact balance of resistances. Hence, at the moment when the battery contact is made, there must generally be an electromotive impulse between Q and P, which will drive a current through the galvanometer coil, and make an embarrassing deflection of the needle if the galvanometer circuit is complete at that instant (as it is in the common plans of Wheatstone's balance), and will require the observer to wait until the needle comes to rest, or until he can tell

\* I make them as little sensible as possible in my coiled testing-conductors, and in sets of coiled standards of resistance, by either doubling each coil or each branch of each coil on itself, or by reversing the lathe at regular intervals in winding on any single coil on a bobbin,—a plan which has also the advantage of rendering the direct electro-magnetic action of any coil so wound very small or quite insensible on any galvanometer needle in its neighbourhood.

precisely to what point its oscillations converge, the current being kept flowing all the time, before he can discover whether the balance of resistances has been attained or not. This absolutely precludes very refined testing, since, whether by the heating and consequent augmentation of resistance of some part of the balanced branches, or by thermo-electric reactions consequent on heating and cooling effects at junctions of dissimilar metals when the branches of the balance are not all of one homogeneous metal, or last, though not least, by the eye losing the precise position where the galvanometer needle or indicating image rested, it is not possible to use the full sensibility of the galvanometer for testing a zero if its needle is allowed to receive such a shock in the course of the weighing. Embarrassment from this source is completely done away with by using the double spring key described above, and giving time, from its first to its second contact, to allow the electro-magnetic induction to subside. An extremely small fraction of a second is enough in almost all cases; and the operator may therefore generally press the key home almost as sharply as he will or can. But when there is a large "electro-dynamic capacity"\* in any part of the balance-circuit, as, for example, when the coil of a powerful electro-magnet with soft iron† core is the conductor whose resistance is tested, it may be necessary to keep the key in its middle position for a few seconds before pressing it home, to avoid obtaining what might be falsely taken for an

\* This term I first introduced in a communication "On Transient Electric Currents" (Phil. Mag., June 1858), to designate what for any electric current through a given conductor is *identical* in meaning with the "simple-mass equivalent" in the motion of Attwood's machine as ordinarily treated. A rule for calculating the electrodynamic capacity is given in that communication; also the rule, with an example, in Nichol's Cyclopædia, article "Magnetism—Dynamical Relations of."

† Giving a resistance to the commencing, to the ceasing, or to any other variation in the strength of an electric current (precisely analogous to the effect of inertia on a current of common fluid),—which it seems quite certain must be owing to true inertia (not of what we should at present regard as the electric fluid or matter itself flowing through the conductor, but) of motions accompanying the current, chiefly rotatory with axes coinciding with the lines of magnetic force in the iron, air, and other matter in the neighbourhood of the conductor, and continuing unchanged as long as the current is kept unchanged. See Nichol's Cyclopædia, article "Magnetism—Dynamical Relations of," edition 1860; also Proceedings of the Royal Society, June 1856; or Phil. Mag., vol. Jan.-June 1857.

indication of too great a resistance to conduction (or "frictional" resistance, as I have elsewhere called it\*), being a true indication of resistance or reaction of inertia to the commencement of the current in the electro-magnetically-loaded branch†. In such cases it is impossible, either by electrodynamic balance or in any other way, to obtain a measurement of resistance without keeping the battery applied for the few seconds required to produce sensibly its final strength of current undiminished by inductive reaction, over and above the time required to get an indication from the galvanometer. But, as already remarked, in all ordinary cases, the inductive reaction becomes insensible after a very small fraction of a second, and the operator may press the double key home to its second contact almost as sharply as he pleases. With such a galvanometer as I have described, he need not hold it down for more than  $\frac{1}{7}$  of a second (the time of the simple vibration of the needle‡) to test the balance of resistances. The order of procedure will therefore generally be this:—The operator will first strike the key sharply, allowing it to rise again instantly, adjust resistances in the balance-circuit according to the indication of the galvanometer; strike the key sharply again, readjust resistances; and so on, until the balance is nearly attained. He will go on repeating the process, but holding the key down rather longer each time. At the last he will press the key gently down, hold it pressed firmly for something less than a second of time, and let it rise again; and if the spot of light reflected from the mirror of the galvanometer does not move sensibly, the resistances are as accurately balanced as he can get them.

\* "Dynamical Theory of Heat, Part VI., Thermo-electric Currents," Transactions of the Royal Society of Edinburgh, 1854; and Phil. Mag. 1856.

† It is probable that a Wheatstone's balance, perfectly adjusted for equilibrium of resistances to conduction, and used with the galvanometer circuit constantly made, so as to show the whole effect of the inductive impulse, may afford the best means for making accurate metrical investigations on electro-magnetic induction, and especially for determining "electrodynamic capacities" in absolute measure.

‡ The mirror galvanometers commonly used in Germany have all much longer periods (ten or twenty times as long in many cases) for the vibration of their needles, and want proportionately longer contacts to obtain full advantage of their sensibility,—in each case a contact during a time equal to that of the vibration of the needle one way being required for this purpose.

VIII. "On the Heat which is developed at the Poles of a Voltaic Battery during the passage of Luminous Discharges in Air and in Vacuo." By JOHN P. GASSIOT, F.R.S. Received June 12, 1861.

1. When the wires attached to the terminal plates of an extended series of a voltaic battery are brought into contact with each other, the circuit of the battery is completed; and if in this state the ends of the wires are separated from each other, the usual luminous or arc-discharge is produced, the length of the arc depending on the number of the cells of which the battery consists. If this luminous discharge is continued for a few seconds, the metallic positive pole or anode becomes red-hot, and will ultimately be fused, while the negative remains comparatively cool. This experiment was originally described by me in the 'Philosophical Magazine' of December 1838, p. 436. In the same periodical of June 1840, p. 478, Mr. Grove suggested as an explanation, that this effect "might be due to the interposed medium, and that, were there any analogy between the state assumed by voltaic electrodes in elastic media and that which they assumed in electrolytes, it would follow that the chemical action in the positive electrode in atmospheric air would be more violent than at the negative, and that, if the chemical action were more violent, the heat would necessarily be more intense."

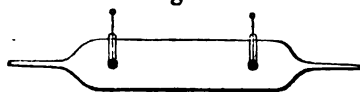
2. Since that time I am not aware that any other explanation relative to the heating of the positive pole of the voltaic battery has been published. Mr. Grove merely gives it as a suggestion; but as it is immaterial whether either or both poles are of copper, aluminium, platinum, or any other metal, or of coke, as in either case it is the positive that is heated, the phenomenon cannot arise from any effect of oxidation, but must be due to some action in the battery circuit hitherto unexplained.

3. In the Bakerian Lecture for 1858, I have stated that, "when the discharge from an induction coil is taken in air or in vacuo with thin platinum wires, the negative terminal becomes red-hot, and if the discharges are continued the wire will be fused." This heating of the negative terminal, provided the wires are thin, always takes place whatever may be the length of the discharge or the medium through which it passes.

4. That this heating of the negative terminal in the discharge from an induction coil had some intimate relation to the heating of the positive pole of the voltaic battery was very probable; but why in the one case the heat should be evolved at the negative, and in the other at the positive terminal, appeared extraordinary and well worth further investigation. The result of the experiments I have made with this object forms the subject of the present communication.

5. After verifying the fact that the heating of the negative terminal of an induction coil is always obtained either in a carbonic acid vacuum, in rarefied gases; or in air, provided the terminal wire is thin, but not if thick, I proceeded to examine with greater precision than formerly the nature and character of the luminous discharges in vacuo as obtained from my water-battery, as well as from the 400 insulated cells of the nitric-acid battery (Proceedings, March 15, 1860); and for this object I had several small vacuum-tubes constructed, about 3 inches long and 1 inch diameter; in each of these, two metallic or carbon balls, about  $\frac{1}{8}$  of an inch in diameter, were attached to the platinum wires, hermetically sealed in the tube about one inch apart.

Fig. 1.



Each wire is protected by a glass tube as far as the ball; the vacuum is obtained by means of carbonic acid absorbed by caustic potassa, as described in my former communications.

6. The discharges in these tubes from an induction coil or from my water-battery present nearly the same appearance, viz. a brilliant luminosity surrounding the negative ball, generally without (fig. 2), but sometimes with (fig. 3) a minute stratified discharge from the positive.

Fig. 2.



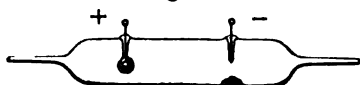
Fig. 3.



7. When the vacuum-tube is introduced into the circuit of 400 insulated cells of the nitric-acid battery, the discharge at first always

assumes the form represented in fig. 2. This discharge, as well as those from the induction coil and the water-battery, when examined, is found to be very perceptibly intermittent, and will generally continue for some time after the circuit has been completed. As the action of the battery improves, *the luminous glow round the negative metallic ball gradually increases in size*, and in a few seconds the ball becomes red-hot. This result I repeatedly obtained; and in two instances with tubes in which balls of aluminium,  $\frac{1}{4}$  inch diameter, had been inserted, the negative dropped from the wire into the tube in a molten state, but leaving the positive ball with its original metallic lustre.

Fig. 4.



8. The preceding experiments, so far from assisting me in explaining the cause of the heating of the positive pole of the voltaic battery, rather tended to complicate the inquiry; for experiment now showed that in carbonic acid vacua the heating effect was elicited at the negative pole, whether the discharge was made from a voltaic battery or from an induction coil; but as experiment also showed that heat is always evolved from the latter at the negative, whether in air or in vacuo, and that from the battery it always in air appears at the positive, it was evident the media through which discharges were made afforded no explanation for the elucidation of a phenomenon of which hitherto the more it was examined by experiment the more difficult appeared the solution.

9. My next experiments were made in vacuum-tubes in which balls of carbon were inserted in lieu of metal. With these I at first obtained the usual heating of the negative, but in one instance I observed that both balls were at the same time exhibiting red heat; in this instance, either from some alteration having taken place in the vacuum, or from some other cause which I had not time to examine, the discharge from the carbon balls became so uncertain as to afford me little information worth recording, except as to the fact I have stated of both balls being heated: the cause of this I was subsequently enabled to determine.

10. In two of the vacuum-tubes hollow brass balls had been attached to the platinum wires. In the first of these the negative very

soon became heated by the discharge. As the discharge of the battery continued, a sudden flash of light was visible in the vacuum, and the glass instantly became coated with metal. On examining the tube, I ascertained that one-half of the negative ball was separated from the other, and partly fused : the intense heat had vaporized the silver with which the two hemispheres forming the ball had been soldered, and it was this vaporized metal that was deposited on the sides of the tube. The positive ball remained much tarnished, as if it had also been heated.

11. I then introduced the second tube, having similar hollow brass balls, into the circuit of the battery. In this tube the negative also was at first heated to redness ; when in this state, a sudden brilliant stratified discharge took place, as fig. 5.

Fig. 5.



The evolution of nitrous acid fumes in the battery denoted its intense action. This discharge from the battery continued for about two seconds, *the negative ball instantly losing its luminosity, the positive becoming red-hot.*

12. This experiment appeared to offer an explanation as to the cause of the heating of the poles ; but on attempting to repeat it, the tube was unfortunately fractured by the heating of the platinum wire, as were also two tubes with coke balls : the result I had obtained was, however, too interesting to me not to repeat the experiment ; and I had six vacuum-tubes prepared, each of the same dimensions as already described, two with coke balls, two with iron, and two with copper, all solid : the best and most conclusive results were obtained with the copper. The experiments which I now proceed to describe were made on the 27th of last May, in the presence of the Rev. Dr. Robinson and Professor Stokes.

13. In the first tube the negative ball very soon became red-hot, when suddenly the brilliant, clearly defined, conical stratified discharge as in fig. 5 appeared, continuing for three or four seconds. On breaking the circuit of the battery, we ascertained that the negative ball, or that attached to the zinc terminal of the battery, had, as before, entirely lost its heated appearance, while the positive, or that attached

to the platinum end of the battery, remained at a dull red heat, which it retained for two or three seconds after the circuit of the battery had been broken.

14. A second tube with copper balls was then introduced into the battery circuit; in this I also obtained the usual luminous negative glow discharge surrounding the ball attached to the zinc terminal of the battery; this glow, as before, gradually enlarged, and in a few seconds the ball became red-hot; the circuit of the battery was then instantly broken, the negative ball retaining its red heat for several seconds. In this experiment it will be observed that the circuit of the battery was broken before the sudden brilliant discharge, previously described, had appeared. These results fully confirmed my original experiment (11) of the alternate heating of the positive and negative terminals of the voltaic battery, when the discharge is made in vacuo.

15. The result of my previous experiment (11) having been thus confirmed, the explanation of those that preceded it became easy of solution. When both coke terminals (9) were heated to redness, the continuous or arc discharge had only lasted for an instant, evolving heat at the positive terminal; but the negative during this short interval retained the luminosity it had previously acquired, and consequently at the time of the disruption of the circuit, both coke terminal balls exhibited red heat. Again, in the first experiment with the brass balls (10), the arc discharge was also only momentary, a disruption of the circuit having been caused by the partial fusion of the negative ball; but the duration of this discharge was sufficient to tarnish the positive terminal; while with balls of aluminium (7), a metal that fuses at a very low temperature, the negative was melted by the heat evolved in the intermittent discharge; and we ascertain that under this condition the positive terminal is in no way affected, for the metal retained its original lustre.

16. The voltaic discharge of a battery of 400 insulated cells, charged with nitric and sulphuric acid, when observed in these vacuum-tubes, at first assumes precisely the same appearance as that of one of nearly 4000 cells charged with rain-water, as it is also similar to that of the discharge from an induction coil (fig. 2); but the action of the nitric acid so far differs from that of the water-battery, that while the discharge of the latter will remain for several weeks (or until the water in some of the cells has evaporated) of nearly the same form



and appearance (figs. 2 or 3), the discharge from the nitric-acid battery quickly alters : as the action of the battery improves, the glow round the negative terminal enlarges, heat in that portion of the tube is sensibly developed, as can be ascertained by touching the tube with the hand, and in a few seconds the negative ball becomes red-hot. During this time no apparent sign of chemical action takes place in the cells of the battery ; the heating effect appears to be mainly due to the elevation of tension ; for if the copper wires attached to the terminals rest on gold-leaf electrosopes, the leaves continue expanded until the arc discharge takes place, when they instantly collapse, and heat is evolved at the positive terminal.

17. In vacuo, as long as the intermittent discharge continues, resistance apparently takes place at the negative terminal ; and this is not only evinced by the heat which is evolved, but by the disruption of the metal which is separated from this pole in minute particles, and deposited in a lateral direction on the sides of the vacuum-tube.

18. As soon as the action of the battery becomes sufficiently energetic, so as to cause the continuous or arc discharge to pass, an entirely new effect is developed : the discharge itself becomes intensely vivid, the stratifications assume a conical form with their apex directed towards the negative (fig. 5), and heat is instantly perceptible at the positive terminal, while the negative as instantly cools.

19. It is beautiful and interesting to observe the suddenness with which the red heat of the negative terminal ball disappears, and the equal suddenness with which the heat is at the same instant elicited in the positive, when the brilliant arc discharge takes place.

*From the results obtained by these experiments, I infer that the development of heat, either at the positive or the negative pole of a voltaic battery, is entirely due to the amount of resistance which takes place in that part of the battery circuit.*

IX. "Contributions to the Physiology of the Liver.—The Influence of an Acid in producing Saccharine Urine." By F. W. PAVY, M.D. Communicated by Dr. SHARPEY. Received June 13, 1861.

In 1854 I conducted an experiment to determine the effect of depriving the blood of its natural alkalinity. I then wished to see if I could influence the presumed destruction of sugar in the blood during its transit through the lungs. Phosphoric acid (*Pharmacopœia* strength), to the extent of  $7\frac{1}{2}$  fluid drachms, was injected into the jugular vein of a dog, and the blood of the arterial system became strongly charged with sugar. Since this experiment was performed, it has been ascertained that there is not the difference during life in the blood on the two sides of the lungs that was formerly supposed to exist; and it has become evident that it was not to arresting any change in the lungs, as I at first supposed, that the result I obtained was due. The fact of the blood being rendered saccharine, led me to infer that a saccharine state of the urine might also be occasioned; and in order to settle this point I undertook the experiments that form the subject of my present communication.

In seven instances I have tried the effect of injecting phosphoric acid into the general circulation. I find that it is an experiment which requires to be performed with the greatest nicety: the animal withstands the introduction of the acid to a certain extent without manifesting any disturbance, and it may even, at first, be injected pretty rapidly; but when a certain amount, varying in different instances according to the size of the animal, has been introduced, the further introduction requires to be made most slowly and with the utmost care, attentively watching its effects; otherwise destruction of life will be occasioned. I have found this to occur in one case after one ounce had been employed, and in another after 10 drachms: in both of these cases the animals were of a smallish size. In two good-sized animals the injection of an ounce did not thus lead to immediate death, and did not produce any alteration in the state of the urine as far as regards sugar. Having carried the experiments further, however, I find that when the injection is pushed to the fullest extent that the animal will safely bear, a saccharine state of the urine is the result. I will give the particulars of two experiments in which

this occurred. I may mention, in passing, that where I have had occasion to give chloroform after the injection of an acid into the circulation, I have found the animal most easily killed by a very small dose, and artificial respiration not of its usual avail in restoring animation.

A good-sized healthy dog which had not been fed the day of the experiment: the urine withdrawn gave no indication of the presence of sugar:  $1\frac{1}{2}$  ounce of the Pharmacopœia phosphoric acid diluted with an equal quantity of water injected slowly into one of the jugular veins. The injection was made so slowly that it occupied half an hour in being effected. In one hour's time from the completion of the injection, the urine was highly charged with sugar, giving a copious orange-red reduction with the copper solution.

A large and strong terrier-dog, not fed since twenty-four hours previous to the experiment: 2 ounces and 2 drachms of phosphoric acid diluted with an equal quantity of water, introduced slowly into one of the jugular veins. This was as much as the animal would bear without seriously endangering its life. The urine, twenty minutes after the completion of the injection, was slightly charged with sugar, and one hour and three quarters afterwards pretty highly so.

It is thus evident that when a sufficient quantity of acid is introduced into the circulatory system, the operations of life are so altered that sugar appears in the blood to such an extent as to occasion a strongly marked saccharine state of the urine. From all that I have seen, it appears to me reasonable to conclude that this result is due to a perverted condition of the processes belonging to the liver. The unnatural state of the blood occasioned by the presence of the acid seems to induce the change of amyloid substance into sugar. It seems to promote that change, the result of chemical action, which occurs with such activity after death, and which must be held in abeyance under natural circumstances during life, sugar being found in the system to so scarcely an appreciable extent.

When the quantity of acid used has been large, the blood nearly loses its power of coagulating. In some of my experiments, also, I have noticed that such a morbid state has been induced as to lead to an escape of blood from the vessels during life. I have met with ecchymosis of the liver and stomach, an accumulation of blood in the stomach and intestine, and the presence of blood in the urine.

Looking upon the effect of the acid injection as due to an action upon the liver, it occurred to me that this organ might be much more directly influenced by injecting the acid into a branch of the portal system instead of the jugular vein. It happens, however, that a circumstance occurs to render this mode of experimenting unsuccessful. Although I have never found the acid lead to a solidification of the blood in the vessels when introduced into the general circulation (indeed, as I have mentioned, the effect is the reverse of this), yet when injected into the portal system it causes the blood belonging to this system to solidify and the vessels to become so plugged up that the circulation of portal blood is completely stopped. I have made five experiments in this way; and in all but one the effect referred to has occurred. In three of the cases the acid was used undiluted, but injected very slowly into one of the mesenteric veins; and, in each, under an ounce was used: in the fourth, 6 drachms of the acid diluted with 12 drachms of water formed the injection employed. All the dogs died in less than one hour and a half, and the trunk of the portal vein with its larger divisions in the liver was found plugged up with solidified blood, so as entirely to check the circulation. The tissue of the liver was in places white and hard, as if it had been chemically acted upon by the acid. There was no sugar to be detected in the urine; and the liver, submitted to an ordinary examination after death, behaved in the usual way. From one experiment I learnt that the effect produced by the acid in solidifying the portal blood instantly occurred on the first portion of the injection being made; so that the portal circulation was at once stopped, which would account for the absence of any sugar in the urine.

In the experiment where the circulation remained free,  $\frac{1}{2}$  an ounce of the acid was used diluted with 2 ounces of water. The injection was slowly made. The urine in an hour and a half's time was found to contain a slight amount of sugar. Two hours after the injection, when life was destroyed, the liver was perfectly natural in appearance, and all the vessels were free. There was therefore no engorgement of the spleen nor blackening of the intestine from congestion, as was the case in the other experiments.

Finding this mode of experimenting proved unavailable, I next tried the effect of introducing the acid into the alimentary canal instead of directly into the portal system. Injecting it into the stomach

was of no use, for it very soon induced vomiting and was rejected. Half an ounce of the phosphoric acid injected through a tube passed down the œsophagus I found rapidly to occasion repeated vomiting efforts, the stomach having been previously empty. Resorting to the use of chloroform, I therefore introduced the acid into the duodenum, or some other portion of the small intestine.

I have the record before me of the five experiments of this kind that I have performed. They in the most striking manner show that an excess of acid in the system occasions the production of saccharine urine. The following are the leading points belonging to these experiments.

No. 1. In a medium-sized dog, which had not been fed for twenty-four hours, 2 ounces of phosphoric acid were injected into the upper part of the small intestine in  $\frac{1}{2}$ -ounce portions at intervals of half an hour. Half an hour after the last half ounce had been injected, the life of the animal was destroyed. The bladder was found distended with urine, which gave a strong reaction of sugar. It had been ascertained, that it was previously free from this principle. The liver presented an ordinary appearance and behaviour. There was no plugging up of the portal vessels; indeed the blood, which was collected during the *post-mortem* examination, scarcely possessed any coagulating property.

No. 2. Fourteen drachms of phosphoric acid were introduced into the duodenum of a largish dog which had not been fed since the day previous. In an hour and a half's time there was a large quantity of urine in the bladder, and it presented a strong reaction of sugar.

No. 3. A medium-sized dog three hours after food: 10 drachms of phosphoric acid injected into the duodenum; urine in two hours' time gave an orange-yellow reduction with the copper solution. Ten drachms more were now again injected, and two hours afterwards the life of the animal was destroyed. The urine gave a still stronger reaction of sugar, namely, an orange-red reduction with the copper solution. The liver presented a natural appearance. The blood collected from the heart had undergone no coagulation whatever by the following day.

No. 4. A small dog five hours after food: 4 drachms of phosphoric acid injected into the duodenum. In one hour and a half the animal was killed. The urine did not contain any sugar. The liver looked

natural, and behaved in the ordinary manner. The blood underwent its usual coagulation.

No. 5. A medium-sized dog that had not been fed for twenty-four hours : 30 drachms of phosphoric acid injected into the duodenum, and two hours afterwards the life of the animal was destroyed. There was no urine found in the bladder. The intestine looked black, as if in a state of mortification. Some of the large venous trunks in the liver were plugged up with coagulated blood. The liver, examined a short time after death, gave only a moderately strong reaction of sugar, and none of the presence of amyloid substance.

It will be observed that in the first three experiments saccharine urine was produced by the influence of phosphoric acid introduced into the intestine. In the fourth, no sugar was found ; because, I apprehend, the quantity of acid employed (4 drachms) was too small. The fifth is incomplete, on account of there having been no urine to test ; but this experiment is of interest, in showing that a large quantity of the acid injected into the intestinal canal may produce the same kind of effect, as regards plugging up the larger veins of the liver, as when introduced directly into a branch of the portal system.

From a consideration of all that has been brought forward, it is evident that the presence of an acid in sufficient amount in the system so perverts the normal processes of life as to occasion a considerable production of sugar, which, passing into the blood, escapes by the urine. Such is the fact ; and, as I have already stated, I consider it to be caused by an effect of the acid upon the liver. The chemical disposition of the amyloid substance to transform into sugar is, I conceive, allowed to come into play from the unnatural state of the blood, and thus the result occurs.

In former communications to the Royal Society, I have mentioned that destruction of the superior cervical ganglia of the sympathetic occasions a temporary diabetes, and that this diabetes is prevented by the previous introduction of carbonate of soda into the circulation. In accordance with what might be expected, the injection of the acid and the operation on the sympathetic produce conjointly a saccharine state of the urine, as either does separately. In one case where I analysed the urine, I found 7·3 grains of sugar per ounce, an hour and a half after the operation on the nerve and the injection had been effected.

I consider it a point of interest (which, however, I merely mention

at present without any comment upon it), that I have observed a jaundiced condition produced by the operation on the sympathetic with the injection of the acid. The urine has been deeply tinged with bile, and has given the characteristic play of colours upon the addition of nitric acid. In the experiments with the injection of the acid alone, it has been a matter of constant observation that a flow of bile has been excited into the duodenum and towards the stomach, the pyloric extremity of which has been highly tinged of a yellow colour.

Although a diabetic state of the urine may be thus artificially induced, apparently by the direct chemical agency of an acid upon the liver, yet I am not prepared to say that, beyond the addition of another significant fact to our knowledge upon this matter, any at present available assistance has been gained towards unravelling the nature of the diabetic disease. Possibly in some cases an insufficiently alkaline state of the portal blood may be the cause of a temporary slightly saccharine state of the urine; but from the observations I have conducted upon diabetics, I certainly am not permitted to think that such is the cause of the well-marked diabetic disease. The immediate cause of the production of sugar in idiopathic diabetes, and in diabetes artificially produced by operations upon the nervous system (the sympathetic and cerebro-spinal), still remains an open point for discovery.

Usually in my experiments with the acid injections the liver has been found fairly charged with amyloid substance; but in a few instances an absence of this principle has been observed, although only a short time has elapsed between the injection and the period of destruction of life.

- X. "On the Chemical and Physical Conditions of the Culture of Cotton." By J. W. MALLET, Ph.D., F.C.S., Professor of Chemistry in the Medical College of Alabama. Communicated by ROBERT MALLET, Esq., F.R.S. Received June 4, 1861.

(Abstract.)

This communication embraces the first portion of an elaborate physical and chemical investigation, in which the author has been and is still engaged, upon the scientific conditions involved in the successful agriculture of the cotton plant. To this train of research he has

brought peculiar advantages, holding his professorial chair in the great cotton-growing State of Alabama (North America), having been the editor of the Government Geological Report of that State, and having had placed at his disposal (for experiment and comparison) by the Indian Government, a magnificent collection, specially made, of the various cotton soils and plants of India, as well as the like from other Powers.

The author remarks that, although an examination of the conditions under which the cotton plant may be cultivated with success is one of much interest both in a scientific and economic point of view, yet it is strange that science, botanical, chemical, and climatological, should as yet have supplied so little information with respect to this plant, the most important source of the clothing of man. That, while other cultivated species, many of them of far less general value, have been the objects of careful experimental research—their botanical relation and improvement by hybridization settled—the character and extent of their demands for atmospheric and mineral food ascertained—the soils upon which they thrive or fail analysed—the climatal conditions which favour or impede their growth observed, the culture of cotton under favourable circumstances has, *as an art*, been advanced in one great locality almost to perfection, but without the scientific *principles* upon which the art is based, and by the application of which alone success or failure in any new attempts elsewhere and under new conditions can be predicted. He shows that the immediate cause of this neglect of the science of cotton-culture has been the facility with which the vast and growing demand of the world for cotton has been met by the vast surface of fertile and virgin soil and other favourable conditions of the southern states of North America, yielding wealth to the planter too readily to incite him to inquire much as to the conditions of his success.

Although much virgin soil remains in the southern states untouched by the cotton-planter, the author states that it needs but slight knowledge of the country to discover the vast extent of "worn-out" cotton fields already existing even in the most recently settled states, or to predict a time when the growing demands for the staple must compel, there and elsewhere, the attention of the economist to the scientific aspects of the problem of cotton-cultivation. To fix with exactness the conditions under which the cotton plant



thrives, how far it can be brought to bear unfavourable circumstances, and by what means the latter may be modified to suit its requirements,—such are the “fruit-bearing” objects of the author’s researches; he remarks, however, that in the purely scientific aspect of his subject it is difficult to overrate the interest that attaches to every question touching the sure production of a material that, in its husbandry, manufacture, and consumption, closely concerns so immense a proportion of the human race. In the present paper, that branch of the subject which relates to the *soil* is taken up.

The author in commencing gives a careful summary of all such experiments on American cotton soils as have been recorded prior to his own labours. These are not numerous.

The second chapter describes, by the aid of a large geological and climatological map, the precise geographical boundaries of the regions of American cotton-culture, the geological or agronomic, climatal, and meteorological and general superficial conditions of these great surfaces, and the relations of each of these to the growth and culture of the several varieties or staples, such as “Sea Island” or “long staple,” and “upland” or “short staple,” &c. The latter sort, constituting the vast bulk of the crop, and bearing, in 1858, the ratio of eleven hundred and six and a half millions to only about twelve million pounds of the “Sea Island,” the author deems worthy of prior investigation. In the first instance, limiting himself to this, he discusses carefully in his third and fourth chapters, the choice of district from which the most typical and important soils for examination should be collected, and decides upon the selection of “fair normal specimens of prairie soil” and its underlying subsoil, and to examine them “as minutely and accurately as possible,” believing that thus a clue would be more readily found to the causes of fertility as dependent on soil (for which this region is remarkable), than by a less careful examination of many specimens from this or that locality over so vast a surface. This method, without teaching *all* that we may want to know, is certain most readily to show us the right direction in which to push further inquiry. Seven specimens of soil and subsoil were finally selected for comparison and analysis, mainly from that class of “prairie land” known as “canebrake land,” in Marengo, County Alabama, in lat. 32° 35' N., long. 87° 36' W., the points being marked upon the map accompanying the paper.

The characteristics of this very peculiar soil, its prevailing weeds, and most important Silva and Flora, its average depth, nature of surface, effects upon it of rain and of drought, the form of its water-courses, and general conditions affecting the cotton plant are carefully described, and their bearings on the subject discussed. The examination of these soils was twofold—physical or chemico-physical, and purely chemical or analytical. The methods employed in each are detailed; and in the former, those of Schübler and of Schulze, with modifications by the author, were principally employed, attention being also given to the methods and results of Liebig's recent experiments, and those of others, on the power of withdrawal by soils of saline and other substances from their solutions. The external characters of the soils examined are then described; the real or true specific gravity, and the weight of a given volume in known conditions of moisture and dryness—the contraction in volume on drying from a determinate extreme of wetness—the cohesion of the soil, or adhesion of its particles (in known conditions as to moisture) to each other, are all determined, the last by a method believed new. The adhesion of these soils to the surfaces of iron implements, as ploughshares, hoes, &c., was ascertained in the state of "maximum moisture," as proposed by Schübler, of whose method, however, the author expresses some disapproval.

The next physical condition determined is the absorption of heat from the sun's rays, with tabulated results, both for the absolute maximum temperature attained; and the rate at which the increment takes place. The results indicate the importance of noting the latter, and prove that the statements of Malaguti and Durocher, in opposition to Schübler, that mineral composition has a more important influence than colour upon the thermo-absorptive capacity, and that of sand is greater than that of clay, must be accepted with considerable limitations. Conversely, the author has determined the relative retentive powers of these soils for heat, having intimate relations with the rate at which they lose heat after the sun has become absent, and tabulated his results, which differ considerably, as he notices, from those of Schübler in analogous cases,—the discrepancies remaining after repetition, by the author, of his own experiments. He points out some of the probable causes of this.

He then proceeds to the power of absorption and retention of

water, with tabulated results. The extreme tenacity with which the best cotton soils retain a very large proportion of their water of saturation after lengthened periods of exposure to dry air is remarkable, and the importance of this in the hot climate of cotton-culture is pointed out. In immediate connexion with this point, their permeability, or the rate at which water percolates through these various soils was ascertained, the relations of which to partial rain or dews, and to the desiccation of one mass and species of land in times of drought by others adjoining are important and obvious; and again, in the same relations, the capillarity, or rate at which water is drawn through and upwards in the soils from deep moisture below, was determined. In this part of his labours the author considers with some exactness the nature and measures of the true capillary power of soils, refers to the recent interesting researches of Jamin on the capillarity of porous bodies, and describes some new and peculiar apparatus by which he has determined this for the soils in question, the results being given in several tables. These indicate strikingly one of the remarkable properties due to the extremely fine state of division of these "best cotton soils," on which, in part, their fertility depends, viz. that they draw up moisture from the subsoil with immense power, and therefore from great depths, but yet do so with great slowness; so that in a torrid climate the subsuperficial supply of water fluctuates but little, and is slowly supplied and long in being exhausted in drought; while other soils pump it up rapidly, and as rapidly waste it. This property becomes more important as the distribution of rain, both in season and in space, is more unequal naturally.

The hygroscopic power, or power of absorbing aqueous vapour from the atmosphere, is next experimented upon, and the results are tabulated, and also represented graphically by curves, as are several of the other numerical results.

The author then proceeds to the highly important subject of the absorptive power of the soils for gases directly or indirectly affecting the growth of plants.

Tabulating the results for oxygen, carbonic acid, and ammonia, the most striking result here exhibited is the prodigious power of absorbing ammonia possessed by the dry canebrake soils. This soil condenses 52 volumes (equal to its own) of ammonia, and its

subsoil 64 volumes. It will be remembered that De Saussure found that the most impalpable powder of boxwood charcoal only absorbed 90 volumes equal to itself. Another cause of the extreme fertility of these soils is thus brought into evidence.

From this the experimenter proceeds to the determination of absorption or withdrawal by the soils of inorganic substances in contact with them, and in solution; experimenting on ammonia, chloride of ammonium, sulphate of ammonia, nitrate of potass, phosphate of soda and silicate of potass, and determining the proportions both of acid and of base withdrawn. The methods by which he proceeded are described with reference to each of the above salts. In several cases the acids and bases are not absorbed in the proportions in which they constitute the salts. These very curious and suggestive results are graphically given as well as tabulated. The labours of various other chemists in this direction are referred to and discussed in reference to those of the author.

Professor Mallet then refers to what he denominates the mechanical analysis of the soil, pointing out the necessity, in all agronomic determinations, of finding, by methods admitting of comparison with distant soils, the texture, coarseness, or fineness, &c. of the constituent particles. These results are given in eight consecutive comparable tables. Each soil was separated into six solid portions and the remaining water making up its total weight—viz. into the material passing through sieves respectively offering 36, 100, 400, and 1600 meshes to the square inch, and into suspended matter of two decantations.

The proportion of impalpable material is very remarkable, amounting in the best soils to more than 70 per cent. of the whole. Not a pebble or particle almost, exceeding  $\frac{1}{40}$ th of an inch in diameter, is to be found in those best cotton soils whose comminuted state permits the free pushing out of root-fibres in all directions.

The purely chemical part of the investigation is then proceeded with. The methods employed for the chemical analysis of the soils are given under the heads of water, organic matter, inorganic matter soluble in water, inorganic matter soluble in hydrochloric acid, inorganic matter decomposable by strong sulphuric acid, and that not acted on by this acid. And the results follow in eight tabular statements, but are of too detailed a character to be brought into

an abstract, and without such the discussion that succeeds would not be sufficiently intelligible to be useful. It may be interesting to state that measurements have shown that the average mass of soil interpenetrated by the roots of each cotton plant in Alabama is about 5 cubic feet; within this bulk of soil a sufficient amount of inorganic constituents for the plant must be found naturally, or be artificially transferred to it.

Professor Mallet compares his results with the analyses of Indian soils made by Dr. Forbes Watson, pointing out both similarities and differences.

In concluding his analyses of the soils and subsoils, the author gives also one of the so-called "Rotten Limestone," and of the "Bored Rock," both calcareous rocks of a very friable character that underlie the Cane Brake soils. He concludes his elaborate research with some general deductions from his examination of Alabama cotton soils, and with an appendix, in which he describes the mechanical or agricultural methods employed in that State in the treatment of the same. These are given as follows in the words of the paper.

*General Deductions from the examination of Alabama Cotton-soil.*

In order to draw any useful conclusions from experiments such as the above, upon a soil, the plant to be cultivated must be noticed, as well as some of the modes in which it is affected by climate.

A few remarks upon the cotton plant and its climatal peculiarities must therefore be made here in anticipation of a future part of the paper.

Annual cotton, as cultivated in America, is a plant which attains its principal growth in about four months, although it continues to develop seed and fibre for a much longer period.

The extent to which its roots penetrate the soil has been noticed above, and from this some imperfect idea may be formed of the power which it possesses of drawing upon the earth for nourishment, although no measure is thereby obtained, I think, of the capacity of the earth to *yield* nourishment\*, as is shown by the experiments upon capillarity and saline absorption. Nor, indeed, do we even obtain any cer-

\* On the assumption of Liebig—that mineral food is taken up only by direct contact with the roots—the surface exposed by the latter does become a measure of the capacity of any particular soil to yield such nourishment.

tain knowledge of the power with which the plant takes up its food from the soil, unless, by microscopic examination, and by experiments such as those of Hales, some estimate be formed of the combined effects of capillary and osmotic action in drawing up liquids of various composition.

The special mineral food required by the cotton plant, and the amount of this food, remain to be examined by analyses of the ash, which will form another part of the investigation. Some statements with regard to the *nature* of the mineral constituents have been made, drawn from the results of Dr. Jackson's analyses, and all these substances needed by the plant have been seen to exist in the soil. As to the *extent* to which they are withdrawn from the soil by cultivation, it may be remarked generally that cotton is by no means an exhausting crop under proper management.

The great mass of the plant—root, stem, branches, leaves, and emptied bolls—remains upon the field, and is ploughed into the soil, which is enriched by the rapid decay of the organic matter. Nothing is removed except the fibre and seed, and a large proportion, if not the whole of the latter, is by judicious planters\* returned to the land; cotton seed is in fact almost the only material used as *manure* in the cotton region of America; a large amount is added to the soil by the ordinary mode of planting, the seed being thickly strewn by handfulls in a continuous row, upon which, after thinning, but a few plants are allowed to remain. The cotton fibre, which constitutes the saleable product, and is absolutely carried off from the land, must be looked upon as a very light crop; a bale of 400 or 500 lbs. to the acre is sometimes obtained under favourable circumstances, but this is much above the average for upland cotton. The fibre yields 1 or  $1\frac{1}{2}$  per cent. of ash, so that at the most  $7\frac{1}{2}$  lbs. of mineral matter per acre will be removed from the soil annually.

According to Johnston (Lectures on Agricultural Chemistry, p. 216), a crop of wheat of 25 bushels to the acre removes from the soil, *in the grain alone*, about 17·65 lbs. of mineral matter; a crop of barley of 38 bushels carries off, *in the grain*, 46·98 lbs.; a crop of oats of 50 bushels, *in the grain*, 58·05 lbs. According to Liebig

\* The practice of selling cotton-seed from the plantation is one to be strongly deprecated; it is beginning to be common in some districts, owing to the increasing manufacture of cotton-seed oil and exportation of the cotton seed-cake to Europe.

(Letters on Modern Agriculture, p. 41), an average crop of potatoes removes from each acre about 163 lbs. of mineral matter ; and one of beet about 458 lbs. (leaves included).

With respect to climate, cotton needs a high summer temperature ; although not properly a tropical plant, it produces fibre in diminished quantity, though of improved quality, when removed from a southern locality to one further north ; it never seems to be directly injured by the most intense midday heat ; when other crops, including even Indian corn, are drooping under a blazing sun, the large succulent-looking leaves of a cotton-field will but seem to enjoy the congenial temperature. As is said by the writer of a pamphlet published by the Cotton Supply Association—“*cotton is decidedly a sun-plant.*”

The proper supply of moisture is a point of at least equal importance with temperature, and here appears undoubtedly to lie the main difficulty hitherto experienced in attempts to extend the culture of cotton into new regions. Published statements differ greatly as to the effect of moisture or dryness upon the plant, some writers saying that a wet season is ruinous to cotton and that drainage is of the first importance ; while others, especially many of those treating of cultivation in India, insist that irrigation is more necessary than anything else. Dr. Royle\* well says, “such terms as moisture and dryness are so entirely comparative, that in one country we hear the cotton plant described as one requiring moisture, and in another we find it stated that no plant requires so little ; the fact being, that the plant can bear both great heat and considerable want of water, provided it is growing in a not over-dry atmosphere.”

The last sentence states an important part of the truth, but, I think, not the whole ; it draws a distinction between *two* forms in which moisture may be supplied to the growing plant, whereas it would seem that *four* should be separately noticed.

1°. The atmosphere may contain a greater or less amount of water in the state of vapour, up to the point of so-called saturation.

2°. The atmosphere may be supersaturated, or in other words, precipitation of liquid water, as rain, &c., may take place from it.

3°. The *soil* may contain a greater or less amount of water intimately united with it, whether by adhesion or chemical combination

\* Dr. J. Forbes Royle ‘On the Culture and Commerce of Cotton in India and elsewhere,’ p. 223.

—such water as is rapidly absorbed from the air by artificially dried soil, and can afterwards be expelled only by the application of a high temperature. This water does not render the soil moist to the touch. It can accumulate in a particular soil to a certain extent only, and this limit may be called the point of saturation of the soil.

4°. The soil may be supersaturated, that is to say, liquid water, evident to the senses as such, may mix with the earth and render it, in the common sense of the term, moist or wet.

Now it would seem that the larger the relative amount of water in the 1st and 3rd of these forms taken up by the cotton plant, and the smaller the quantity received in the 2nd and 4th forms (at least during the greater part of its period of growth), the more favourable will be the result.

In water-soaked soil, *i. e.* holding water in the 4th condition mentioned, cotton will not thrive. The following statements\* are borne out by the general experience of planters. "The tap-root of the cotton plant will not strike down into wet soil. . . . On wet land the cotton plant grows small, looks sickly, or *scalds* in the hot sun, and bears but little raw cotton, and it takes twice the labour to cultivate it, as the grass usually grows the faster, and is much more difficult to kill out." Such soil will obviously be benefited by draining. On the other hand, the state of things demanding artificial irrigation—very necessary probably in some parts of India—would seem to be simply the absence of water in any one of the three other conditions noticed.

In the early stages of growth the plant receives a moderate supply of rain, *i. e.* water in the 2nd condition named, with advantage; but even then heavy rains are very injurious, and later in the season they are absolutely destructive; the bolls do not open, but fall off, or rot upon the branches—a surface growth of grass and weeds accumulates so rapidly as to choke the crop—the boll worm and other destructive insects make their appearance—and the cotton from bolls already open hangs out in trailing locks, draggled, dirty, and matted together. Dry years are emphatically those of the largest and best crops.

Yet, like all other plants, cotton must be supplied with moisture;

\* From the pamphlet 'On the Cultivation of Orleans Staple Cotton,' published by the Manchester Cotton Supply Association, p. 14.



this even seems to be needed in considerable quantity, judging from the extensive leaf-surface from which evaporation is carried on. Aqueous vapour in the air, as suggested by Dr. Royle, and abundant hygroscopic moisture in the soil itself, as I would add, seem to be decidedly the sources from which this requirement is to be met.

The great advantage derivable from a soil of high capacity for absorbing and retaining moisture will be, that it will enable the plant to withstand vicissitudes of weather and season; in damp weather hygroscopic water will be condensed, to be stored up in the retentive soil until required in the midst of drought; in August or September, when seed and fibre are to be formed, and when therefore diminished leaf-activity is desirable, the roots will gradually draw up a supply of this water, limited\*, but sufficient to maintain healthy life.

These remarks no doubt apply also to the absorption of gases, and of mineral matter withdrawn from solution, as has been noticed in a previous part of the paper. The power of steadily accumulating and gradually dispersing seems to be one of the well-marked and beautiful provisions of the "*economy of nature*."

The soil of the prairie region of central Alabama fulfils the above conditions admirably, and to this fact should, I believe, be in large measure attributed the success with which cotton is cultivated upon it.

To sum up the results of the examination of this fertile cotton soil, it is shown to be a stiff aluminous clay, containing moderate amounts of organic matter and of the mineral substances needed by the plant as food—of great uniformity, and in an exceedingly fine state of division—above all, possessing a very high capacity for absorbing and retaining heat, moisture, gases, and soluble mineral matter.

#### APPENDIX.

##### *Mechanical Treatment of Cotton-soil, as practised in Alabama.*

In order to complete the subject of Alabama cotton-soil, it seems desirable that to the preceding results should be added a brief state-

\* The porous chalky substance referred to as "rotten limestone," which underlies the soil of the cane-brake, has itself very similar capillary and absorptive power; it is penetrated by sandy strata, through which water can readily flow, and hence it is not at all inconceivable that supplies of moisture may even through this be drawn up from the depth of 20 or 30 feet, at which the first sandy bed is often met.

ment of the way in which this soil is usually cultivated, which may easily be done in a few paragraphs.

As early in the winter as the weather is favourable and the condition of the ground suitable, *i. e.* when not too wet\*, the preparation for the crop commences by "bedding" the land. This is done by running a narrow plough called a "bull-tongue" at regular intervals across the field, the common distance being four feet between the rows. In very rich alluvial land the distance is sometimes five or even six feet, and in thin poor land not more than three feet. Upon the furrow thus produced the ridge or "bed" is made by ploughing to it on either side with a turning plough, called a "Carey" plough, drawn by two mules or horses, until the space between the rows has been ploughed out. The whole field is in this way thrown into ridges, which should run horizontally round any elevated portions of the plantation, so that heavy rains may not wash away the soil.

When the time for planting arrives—about the beginning of April—a furrow is run along the top of each ridge by a narrow plough, and in this furrow the cotton seed is pretty thickly strewn by handfulls as the labourer goes along the row. It is then covered with earth by a heavy wooden block, which a mule or horse draws along, so as not only to cover up the seed, but to press the earth firmly upon it.

If the weather be favourable for the germination of the seed, it comes up in ten days or a fortnight, and soon afterwards the cultivation commences by thinning out the cotton with hoes, so as to leave but a few stalks together at intervals of eight or ten inches, removing also any grass or weeds which may have grown with the cotton. The space between the rows is at the same time ploughed to make the earth light and mellow, and to destroy grass and weeds. Great skill is shown by trained negroes in the use of the plough and hoe,—the former is often run within two inches of the cotton, and the latter used to cut out a weed within an inch or even half an inch, without in either case injuring the cotton itself. The process of working the crop with plough and hoe is continued at intervals of three weeks, and at each time of thus going over the field the cotton is thinned out, until it is brought to a "stand," that is, reduced to single

\* The peculiar condition of the "cane-brake" soil (to which the above remarks apply) intermediate between a dry and a thoroughly wet state, in which alone ploughing can well be carried on, has been noticed in an earlier part of the paper.

stalks 12 or 18 inches apart upon the rows. Early in July the plant has usually acquired sufficient size to shade the ground and prevent the further growth of grass, and the crop is then "laid by." It is of the first importance that the land should have been kept perfectly clear of weeds up to this time, and in a hot climate the task is often a difficult one.

Simultaneously with the cultivation of cotton, the cultivation of Indian corn, sweet potatoes, &c., proceeds, in order to furnish food for the negroes of the plantation, for the mules or other draught animals, and for a sufficient number of hogs to yield meat for the labourers.

In middle Alabama the cotton plant usually commences flowering early in June, and continues to bloom until frost kills it, bolls continuing therefore to form during several months. The earliest bolls open, in ordinary years, from the 10th to the 15th of August, when the "picking" season commences. This lasts until the cotton is all gathered—until January or even February, if a full crop be made. The bolls continue gradually to open long after frost has prevented the formation of any more. In picking the cotton from the boll, surprising manual dexterity is shown by negroes accustomed to the task from early youth.

The seed cotton, as it is collected into large baskets by the pickers, is carried to the "gin-house" of the plantation, and "ginned;" and when enough of the clean fibre has accumulated, a day or two is devoted by a part of the hands to pressing it with the large wooden "screw" into bales ready for shipment by river to the sea-port.

XI. "Account of Experiments made at Holyhead (North Wales) upon the Transit-Velocity of Waves through the Local Rock Formations." By ROBERT MALLET, Esq., C.E., F.R.S.  
Received June 18, 1861.

(Abstract.) •

These experiments were made by the author at the joint request of the Royal Society and of the British Association for the Advancement of Science, aided by grants from each of those bodies.

Their object was to ascertain the transit rate or velocity of propagation of waves of elastic compression, analogous to those of na-

tural earthquakes, through the stratified and highly convoluted, laminated, and shattered slate and quartz rocks of the neighbourhood of Holyhead, where the recurrent explosion of very large mines of gunpowder in the Government quarries (for the obtaining of rock for construction of the Asylum Harbour) afforded a valuable opportunity in the production of the requisite impulses for generation of the wave.

The author had previously determined experimentally (at Killiney Bay, in Ireland) the transit velocity of such waves in wet sand, and in highly-shattered and more solid granite; media presenting, probably, the extremes of slowness and of fastness of wave transit. It was still desirable to determine this for rock, not only minutely crystalline, but also stratified, convoluted, and generally highly perplexed and heterogeneous in internal structure. The instrumental means employed were generally similar to those adopted previously at Killiney Bay, with suitable modifications consequent upon the great charges of powder fired, which at these quarries have reached as much as nine tons at a single blast or mine. The seismoscope (see *Trans. Brit. Assoc.* 1851, *Second Report on Facts of Earthquakes*, R. Mallet, p. 277, &c.) was placed upon a levelled table of solid rock at a suitable station (shown on the map and sections that accompany the paper), and with it the chronograph and galvanic apparatus, by which, on making contact, by the author's pressing his hand upon the lever of the latter instrument, the mine at the quarry, distant in all cases about a mile, was ignited, and the time that elapsed between the starting of the elastic wave from the impulse of the explosion to its arrival at the observer and visibility in the field of the seismoscope was recorded. This registered time was subject to three principal corrections, the respective coefficients of which are also determined experimentally. The instruments admitted of time determinations to within nearly  $\frac{1}{1000}$  of a second. The range over which the wave traversed was accurately obtained in length for each separate experiment. A constant distance from the observing station = 4584·80 feet, up to a fixed point near the quarries, was obtained with precision, in the first instance, by trigonometrical operations, upon a measured base of 1432 feet. The distance of the mean centre of each mine or heading was subsequently measured in a right line to this fixed point, and the angle made by

the latter with the former line determined, whence the direct distance between the mean centre of each particular "heading," or mine, and the observer's station, was trigonometrically deduced.

The following table gives part of the results obtained from six good experiments.

TABLE.

No. of experiments.	Weight of powder exploded.	Total distance of mean centre of heading from observer.	Total observed time of transit.	Observed rate of transit per second, uncorrected.	Final corrected transit rates observed.
	lbs.	feet.	seconds.	feet per second.	feet per second.
1	3,200	6582·93	7·346	896·12	1016·200
2	2,100	5476·57	5·658	967·93	1098·958
3	2,600	6377·14	6·524	977·26	1109·483
4	6,200	6403·48	5·455	1173·87	1331·168
5	12,000	5038·13	4·161	1210·79	1373·035
6	4,400	5228·59	5·249	996·11	1129·598

One very remarkable result is at once apparent on inspecting this table—viz. that the transit rate tends to increase in velocity with the increased quantity of powder fired; in other words, that the loss of velocity in the same rock is less in some proportion as the force of the originating impulse is greater, and so its amplitude greater at starting. This is seen if the experiments be arranged in the order of increased weight of powder.

No. of experiment	2.	3.	1.	6.	4.	5.
Weight of powder	2100 lbs.	2600 lbs.	3200 lbs.	4400 lbs.	6200 lbs.	12,000 lb.
Uncorrected transit velocities . .	967·93	977·26	896·12	996·11	1173·87	1210·79

Experiment No. 1 forms the only apparent exception, and even there the departure is not large.

This fact, now for the first time (so far as the author knows) experimentally proved, appears remarkably in coincidence with the theoretical researches of Mr. Earnshaw.

The general mean transit velocity derivable from all the experiments taken together gives 1176·407 feet per second for the rate. The results, however, obviously form two groups—viz. Nos. 1, 2, 3 and 6 from the smaller, and Nos. 4 and 5 from the greater charges of powder. The mean from the first four is 1088·5597 feet per second; that from the two last 1352·1015 feet per second; and

taking a mean of means from both, we obtain 1220·3306 feet per second as the mean transit velocity of propagation, in the rocks experimented on, of wave pulses due to the impulse of explosions of not exceeding 12,000 lbs. of powder.

The first mean from the smallest charges is that which must be compared with the Killiney Bay experiments. It thus appears that the wave velocity in highly contorted and foliated rock is very low, and is intermediate between the transit rate in wet sand and in discontinuous granite, or

In wet sand = 824·915 feet per second.

In contorted and stratified rock, quartz, and slate = 1088·559 feet per second.

In discontinuous granite = 1306·425 feet per second.

In more solid granite = 1664·574 feet per second.

The general mean obtained, 1220·33 feet per second, or 13·877 statute miles English per minute, co-ordinates, as might be expected, with the carefully made deductions of Nöggerath and of Schmidt from the actual earthquakes of the Rhine and of Hungary, as well as with those of the author from the great Naples earthquake of 1857.

In experimenting with these great explosions the author was enabled to observe, by means of the seismoscope, that the advent of the great wave of impulse (which was sometimes sufficient to make the mercury sway visibly in the trough of the instrument) was preceded by rapidly augmenting tremors, quite like those which very generally precede the great shock in natural earthquakes.

The wave transit in these experiments was made partly in slate rocks and partly in quartz formations, which, though lithologically and geologically distinct, are nearly identical in wave propagative power (as this author has shown by a train of special experiments at the conclusion of the paper), differing not more than in the ratio of 0·576 for the slate to 0·558 for the quartz. The author concludes by pointing out several deductions having interest to general physics, and some of the special relations of the results to Seismology and Physical Geology.

XII. "On the Cutaneous Sensibility of the Hand and Foot in different parts of the Surface, as tested by the Continuous Galvanic Current." By HARRY LOBB, Esq. Communicated by JOHN SIMON, Esq. Received May 28, 1861.

Procure a 60-element Pulvermacher's bath battery, excite it with ordinary vinegar, and hang it up free in the air by a piece of cord. Attach conducting wires to the terminal hooks, and to the wire from the copper or positive pole connect a moistened sponge-conductor, which tie round the neck, so that the sponge may rest upon the skin over the middle cervical vertebra. To the wire, from the zinc or negative pole, attach a conductor of smooth metal.

Upon placing the metal conductor upon the skin of the arm or back of the hand the burning sensation is unbearable for a single moment.

Upon placing it upon any portion of the skin of the palm, or under surface of the fingers, no pain is experienced, nothing but the touch of the conductor.

There is, however, a line running down the side of each finger nearer the back than the palmar surface, where painful sensation commences. This line is also to be traced round, between the thumb and first finger, along the back surface of the thumb and the limit between the skin of the palm and the wrist. This line can be easily detected by the eye; the smooth papillary skin of the palm being insensible, whereas the hairy, polygonally reticulated skin of the back of the hand and wrist is acutely sensible.

The skin on the back of the first phalanges is insensible, and also a part of the way down the second, varying in different persons. If the conductor be placed in the centre of the palm of the hand and held there with gentle pressure for a minute or two, the idea conveyed is that of greatly increasing pressure, as if the instrument was being thrust through the palm, until the pain becomes unbearable; but there is no sensation of burning, pricking, or stinging. The same laws are discovered to hold good in respect to the foot.

XIII. "Experiments and Observations on the Structure and Function of the Stomach in the Vertebrate Class." By WILLIAM BRINTON, M.D., Lecturer on Physiology at St. Thomas's Hospital. Communicated by JOHN SIMON, Esq. Received June 12, 1861.

(Abstract.)

This paper, itself a summary of a long series of observations, may be briefly abstracted as follows :—

The peculiar dimorphous structure possessed by the tubes of the cardiac and middle parts of the stomach in the Dog, exists in the whole Vertebrate class ; about sixty species of which are cited by the author from his notes.

Many of the variations of the stomach throughout the Vertebrate class are essentially œsophageal developments, having a mechanical office.

The more essential contrasts of the vertebrate stomach refer to the above dimorphous structure ; which diminishing, even in Man, to less than twice its thickness in passing from the cardia to the pylorus, is in most animals altogether deficient here, either with or without a great shortening of the columnar-celled tubes.

The dimorphous cell-growth is always the source and exponent of the pepsinous or digestive power of the stomach on protein-compounds ; which power is absent where this structure is deficient, and present (as tested by careful comparison) in the degree in which the mucous membrane contains the dimorphous cell-growth.

Of those alternatives respecting the gastric juice suggested by various experiments—namely (1) the secretion of pepsine by the dimorphous cell-growth, and acid by the columnar ; or (2) the perfecting of the secretion (itself perhaps owing these two constituents to its two kinds of cell respectively) in the open ends only of these tubes—the latter is, on the whole, preferable.

The protection of the stomach from its own secretion is effected mainly by the salivary and other secretions which enter it from the œsophagus and duodenum.

The living stomach decomposes a neutral or alkaline blood so as to set free an acid ; but though transfused blood undergoes this de-



composition, a saline solution not only fails to evoke it, but injures or destroys the gastric powers.

There is no evidence of any exhaustion of the pepsinous constituents of the gastric mucous membrane at any period of digestion.

As regards other variations of power, *age* seems to exercise but a moderate influence on the gastric mucous membrane; *species*, again, has a subordinate effect; *genus* is much more influential. For units of mucous membrane, Fishes seem to have the most powerful gastric digestion.

The influences of temperature, analogous in all animals, vary exceedingly as regards the absolute heats which respectively favour, accelerate, and destroy the powers of the stomach; so that the same heat which is practically necessary for the function of this organ in a warm-blooded Mammal, annihilates the efficacy of the pepsine of many Fishes.

The action of the gastric juice is a transfer, to albumen, &c., of a molecular change going on in the gastric juice; pepsine and peptone being essentially analogous to each other in properties.

The formation of peptone is a hydration of albumen, as shown by various collateral circumstances of the process.

#### ADDENDA.

1. The pancreatic juice, or pancreatic infusion, which converts albumen into a substance akin to peptone, and in proportions not very unlike those which would be obtainable by using certain parts of the mucous membrane of the stomach of some animals, and which does this by a process so far *sui generis*, as that it is no way shared by the salivary organs most analogous to itself in structure and function, is yet distinguishable from the gastric juice in regard to the process and the product of this change. The change is, indeed, an incident of putrefaction only, and therefore not a function of the healthy living organ.

2. The intestinal juice, or the secretion of the intestinal tubes, is neither capable of converting albumen, &c. into peptone, nor of converting starch into sugar. The offices of these tubes are therefore, probably, chiefly of absorption and of excretion.

XIV. "Notes of Researches on the Intimate Structure of the Brain."—Second Series. By J. LOCKHART CLARKE, Esq., F.R.S. Received June 20, 1861.

In consequence of the frequent interruptions to which I am necessarily exposed in the course of my anatomical investigations, I beg to communicate to the Royal Society, in the form of notes, some of the results at which I have arrived, with a promise to forward, in a few months, a complete memoir on the same subject, with the necessary illustrations.

In my memoir of the "Medulla Oblongata," it is shown that the post-pyramidal ganglion, or grey substance of the posterior pyramid, is developed from the posterior grey substance on each side of the posterior median fissure. At the point of the calamus scriptorius, it is intimately connected with the pyriform nucleus of the vagus nerve, each receiving a process from the other. Between these two, and apparently developed from the substance of both, which are intimately blended with it, there arises a convex and somewhat triangular mass, which becomes the principal nucleus of the auditory nerve. In a transverse section it is triangular, one of its angles projecting forwards into the root of the caput cornu posterioris, or expanded extremity of the posterior horn. It is interspersed with numerous large nerve-cells, which are round, oval, triangular, or otherwise irregular, and of considerable size, the largest measuring the 800th of an inch in diameter. Another portion of the auditory nucleus is in contact with the outer side of that just described, and with the inner side of the restiform body. It consists of the outer part of the posterior pyramid in the form of a remarkable network, enclosing in its meshes longitudinal fasciculi, and containing large nerve-cells with branched and exceedingly long processes, which contribute to form the network. From both these parts of the nucleus the posterior division of the auditory nerve takes its origin, and winds outward as a broad convex band over the restiform body. In this course it contains, at first, a few small cells, elongated in the direction of its fibres; but as it proceeds, its cells become gradually larger and more numerous, until at the anterior border of the restiform body it enlarges into a pyriform ganglion, which is crowded

with nerve-cells similar in appearance to those of the inner nucleus. The nerve is also reinforced by fibres radiating from the centre of the restiform body as it winds round the latter. The *anterior* division of the auditory nerve consists of two portions: the principal portion penetrates the medulla beneath the restiform body, and, running along the outer side of the caput cornu, enters both parts of the auditory nucleus; the other portion runs backward along the upper border of the restiform body, which it accompanies over the superior peduncle to the inferior vermiform process of the cerebellum. The outer portion of the auditory nucleus, consisting of the posterior pyramid and its vesicular network, is also ultimately thrown backward into the cerebellum, part of it arching over the fourth ventricle to the opposite side, while the rest extend outward to the corpus dentatum. The corpus dentatum is grasped, as it were, by an expansion of the restiform body, which bends backward after entering the cerebellum.

It would be useless in this place to attempt an intelligible description of the complicated course of the fibres in the human pons Varolii; but I may observe, that at its lower portion I find a structure in every sense homologous with that which in Mammalia is called the *trapezium*. It is not, however, situated *below*, but *within* the pons, of which it forms the most posterior of the transverse fibres. These transverse or arciform fibres proceed out of the restiform bodies and auditory ganglia, and sweep round the extremity of the caput cornu to the back of the anterior pyramids to decussate across the raphe. As they pass the ganglion, or auditory nerve, they receive fibres from it. On each side, where, in animals, it forms a rounded projection, it contains a very peculiar vesicular nucleus, which has a convoluted appearance and structure, resembling another olivary body. These nuclei, which were first pointed out by myself in 1857 (Proceedings of Royal Society), and subsequently by Prof. Schröder V. der Kolk, first make their appearance much lower down in the medulla, at the point where the other olivary bodies begin to diminish, as two cylindrical columns of large nerve-cells with bundles of longitudinal fibres. On reaching the *trapezium*, the cells become much smaller, and are arranged in convoluted laminæ. They are larger in animals than in Man, and are particularly developed in the Cat. Through the trapezium the facial nerve passes transversely backward and

inward beneath the root of the trifacial nerve and the caput cornu, to a group of large multipolar cells, which lies near the surface of the fourth ventricle, on each side of the median furrow, and which gives origin also to the sixth nerve.

The transverse fibres of the pons Varolii are an extension of the arciform fibres of the medulla oblongata. The grey substance of the pons is arranged in a peculiar manner. Its cells are generally round, oval, and fusiform, of about the 1500th of an inch in diameter, and are so connected with nerve-fibres, in chains or bundles, as to form a complete network. The principal chains of this network have, in general, a longitudinal extension, and follow the course of the longitudinal fasciculi of the anterior pyramids, from the sides of which they send off processes across the transverse bundles, in which, as well as around the longitudinal bundles of the pyramids, the cells are at intervals collected into groups of variable size and shape.

The trifacial or fifth nerve originates from a somewhat conical eminence of grey substance, which is situate immediately behind the fossa where the anterior fibres of the pons Varolii meet the point of the fillet, at the upper edge of the middle peduncle of the cerebellum. This grey mass consists of three distinct nuclei in close apposition. Its outer portion is the caput cornu posterioris, or dilated extremity of the posterior cornu, and is composed of several groups of small round and oval cells, of about the 1500th of an inch in diameter, together with numerous longitudinal bundles of nerve-fibres. The posterior portion of the grey mass overlies, and is continuous with, the root of the caput cornu. It is the upward extension of the inner nucleus of the auditory nerve, and contains many cells of a larger kind. The inner side of the mass consists of an oval group of large multipolar cells of the same shape and size as those of the anterior cornu of the medulla spinalis. The large root of the fifth nerve may be divided into three parts, two of which originate in the conical mass just described; of these two, the larger arises from the caput cornu posterioris, while the fibres of the smaller may be traced backward to the continuation of the auditory ganglion, which overlies the cornu. These latter are probably the fibres which go to form the gustatory branch of the fifth nerve; for it was formerly shown that the commencement or lowest part of the auditory ganglion was the principal origin of the glosso-pharyngeal nerve. The third

division of the large root of the fifth descends towards the medulla, not, as has been believed, in the restiform body, but, as I formerly showed, in the very substance of the caput cornu posterioris, of which it constitutes many of the longitudinal bundles.

The small, or motor root of the fifth arises from the same eminence by three or four parallel bundles, a little anterior to the larger root, being separated from it only by a small process of the middle peduncle coming from the upper portion of the pons Varolii. Its bundles pass obliquely to the inner side of the caput cornu, and spread out their fibres through the oval group of large multipolar cells already mentioned. Some of the fibres of the fifth nerve decussate with those of the opposite side through the raphe.

Some of the fibres which form the point of the fillet, as well as others coming from the anterior border of the pons Varolii, are connected with the grey eminence of the fifth nerve.

In tracing the caput cornu towards the brain, it is found to extend obliquely outwards from the angle in the floor of the fourth ventricle, beneath the superior peduncle: the deep portion of the superior peduncle, before it reaches the corpora quadrigemina, passes from the side of the fillet transversely inwards in front of the caput cornu to the middle line, where it decussates with the corresponding part of the opposite side. Part of the grey substance of the caput cornu becomes imbedded in the fibres of the fillet, and ascends with them; while the rest extends more and more backward along the under part of the superior peduncle, until it arrives at its inner border at the side of the valve of Vieussens. Across the thin posterior edge of the superior peduncle it sends numerous grey processes outwards, to meet similar processes from the grey substance in the fillet; so that the longitudinal fibres of this part of the peduncle are formed into small bundles by a network of grey substance; the testis, or posterior portion of the corpora quadrigemina, on each side, rests upon this structure: it is continuous with, and apparently developed from it.

While these changes are in progress, the nucleus of the motor root of the fifth nerve gradually disappears, and is replaced by a group of cells deeply coloured with pigment, and constituting what is known as the *substantia ferruginea*; at the same time the grey substance which forms the floor of the fourth ventricle on each side

of the median furrow increases gradually in depth, and includes the *substantia ferruginea*. It is developed in the tract which is continuous with the nucleus of the hypoglossal nerve, of which it is the analogue. Anteriorly, on each side of the raphe, there is gradually developed within it a considerable group of large multipolar cells, which becomes the nucleus of the third cerebral nerve. At its outer and back part it adjoins the tract which forms the side wall of the ventricle, is continuous with the auditory nucleus, and lies at the base of the *caput cornu posterioris*. This latter tract, on reaching the corpora quadrigemina, becomes continuous with its fellow of the opposite side to form the roof of the *iter a tertio ad quantum ventriculorum*. Here it becomes gradually broader and more transparent, and, in the form of an arched lamina, supports the lateral lobes of the corpora quadrigemina, with which, at its border, it is intimately *blended*, and which communicate over it by a transverse band of fibres. It contains a multitude of small cells of various shape and size, and is interlaced in every direction by very fine nerve-fibres; but of the latter there are two regular sets, which run longitudinally and transversely, and which increase in number from behind forwards, so that they are much more numerous beneath the nates; the transverse fibres radiate in straight lines towards the surface through the lobes of the corpora quadrigemina.

*Structure of the Corpora Quadrigemina.*—The lateral lobe of each testis is an oval or almond-shaped mass, which rests on the convexity of the transparent arch just described. It contains a considerable number of small and uniformly scattered cells of various shapes, and of an average size of about the 2400th of an inch, together with oblique, transverse, and longitudinal nerve-fibres; the longitudinal fibres form a denser layer at its inner and still more at its outer side. The transverse fibres form two sets, one extending over the arched lamina to the opposite lobe, and derived chiefly from the fillet; the other radiating, as already stated, from the substance of this lamina beneath, towards the surface, and becoming continuous, at least in part, with the first set, as well as with the longitudinal.

The structure of the nates is, in general, similar to that of the testes: their cells are nearly of the same nature, but smaller; their lobes are shallower, but longer; and the arched transparent lamina on which they rest is broader and more transparent. Both the trans-

verse and longitudinal fibres are much more numerous. Those which extend transversely from *side to side* decussate in the middle line, and, through the under half of the lobes, form a dense layer of separate bundles; between these bundles, and at right angles to them, the other set of transverse fibres radiate from the arched transparent substance beneath, and after penetrating to various depths, turn round to become continuous partly with them and partly with the longitudinal fibres which run chiefly along the superficial half of the lobes. The same system of latero-transverse bundles is continued in front of the nates to form the posterior commissure. A large proportion of the optic nerve is connected with the nates. The optic tract, on reaching the back of the thalamus opticus, divides into two parts: one of these, after bending suddenly downwards and somewhat inwards, enters the corresponding lobe of the nates, spreading amongst its cells, and forming its longitudinal as well as some of its transverse fibres; the other division of the optic tract winds inwards and somewhat backwards over the thalamus, which it enters, diverging, between streaks of longitudinal fasciculi, in a series of separate bundles, which, in their turn, diverge and distribute their fibres in every direction amongst a dense collection of cells. The cells are various in size and shape; a considerable proportion are round, oval, or fusiform in the direction of the bundles which first enter. Some of the fibres of the optic tract are connected with the *corpus geniculatum internum*, which is crowded with cells of a medium or rather small size, and for the most part round or oval.

*Structure of the Pineal Gland.*—The structure and relations of this body are of great interest, and deserve close attention. It is attached by its base to the posterior commissure. The arched transparent lamina which forms the roof of the canal, or iter, beneath the corpora quadrigemina, decreases in thickness or depth as it approaches the anterior extremity of the canal, beyond which it is reduced to the epithelial layer which lined it. This epithelium is now reflected round the front of the posterior commissure, and from thence under the base of the pineal gland. When carefully examined, it is found, along its border, to be composed of narrow fusiform cells, intermixed with round and oval granular nuclei, of which the average diameter is about the 2800th of an inch. From both kinds of these cells or nuclei fibres proceed and cross each other in every

direction ; so that they form a considerable layer around the commissure, through which, by numerous fissures, they radiate to its opposite surface. This fibrous and inner layer is crowded with round and oval nuclei like those found at the border. Now when the epithelium is followed to the pineal gland, the substance of the latter is seen to be composed of elements which resemble it so closely, and are so uninterruptedly continuous with it, that one can scarcely question the probability that the two kinds of structures are only slight modifications of each other. The pineal gland consists of fibres, nuclei, and the well-known brain-sand. The fibres are arranged in two ways ; throughout the gland they form, by peculiar subdivisions and communications, an intricate network, in which the nuclei are lodged. Some of them are exceedingly fine, others rather coarse, but bear no resemblance to what we are accustomed to call nerve-fibres. They are frequently crooked, and apparently jointed or dilated at intervals, where they give off branches which bear nuclei ; in other places the nuclei are surrounded by flat riband-like fibres. Here and there the fibres of the network go off to form straight bundles, which unite into larger trunks, and have chiefly a transverse direction. The reticular structure bears a decided resemblance to the epithelium of the olfactory mucous membrane, and still more to what I have elsewhere described as the fourth layer of the olfactory bulb in the Sheep, and particularly in the Cat.

In the pons Varolii the decussation between fibres from the opposite sides is extremely complicated. In a direction from behind forwards there is a series of decussations :—1. Between fibres of the superior peduncles of the cerebellum ; 2. Between some of the inner fibres of the fillet as they descend obliquely to the medulla oblongata ; 3. Between fibres proceeding from the corpora quadrigemina behind the crura cerebri ; 4. Between fibres proceeding from the floor of the iter a tertio ad quartum ventriculum and the nucleus of the third nerve ; and, 5. Between numerous fibres of the crura cerebri, as they enter the upper end of the pons Varolii.

In the substance of the pons Varolii, and resting on the outer and back part of the cylinder formed by the superior peduncle on each side, there is a short but thick cylindrical column of large multipolar cells, pierced by a number of longitudinal bundles, and connected by fibres with the nucleus of the third nerve. It commences



a little below the origin of the third nerve, fibres of which pass directly through it ; but I am at present uncertain whether any of them originate from its cells. It is much more distinct in some animals than in Man.

The corpora albicantia are ganglionic masses containing a multitude of cells, which are mostly round, oval, pyriform, fusiform, triangular, and quadrangular, and lie amongst the winding fibres of the anterior pillars of the fornix. These cells vary considerably in size, but are generally small ; the diameter of the largest of the spherical kind being about the 2000th of an inch. The two bodies are united across the middle line by a transverse commissure of fibres containing cells which are fusiform in the same direction.

Except when stated to the contrary, the above notes refer exclusively to the structure of the *human* brain.

XV. "On the Influence of Atmospheric Pressure upon some of the Phenomena of Combustion." By Dr. EDWARD FRANKLAND, F.R.S. Received June 20, 1861.

(Abstract.)

The author has concluded his experiments upon this subject ; and, in addition to the details of the results which have already been briefly mentioned in the 'Proceedings of the Royal Society'\*, communicates the following :—

Although *the rate of burning* of candles and other similar combustibles, whose flames depend upon the volatilization and ignition of combustible matter in contact with atmospheric air, is not perceptibly affected by the pressure of the supporting medium, yet this is not true of all combustibles. The rate of burning of self-supporting combustibles, like the time-fuses of shells, depends essentially upon the pressure of the medium in which they are deflagrated. Attention was first called to this fact by Quartermaster Mitchell†, who found that the fuses of shells burnt longer at elevated stations than when ignited near the level of the sea. The results of the

\* Proceed. Royal Soc. vol. xi. p. 137.

† Ibid. vol. vii. p. 316.

author's experiments with six-inch or thirty-seconds fuses burnt in artificially rarefied air are embodied in the following table:—

Average pressure of air in inches of mercury.	Average time of deflagration of six-inch fuse.	Increase of time of burning over preceding observation.	Reduction of pressure corresponding with increase of time.	Increase of time for each diminution of one inch pressure.
	Seconds.	Seconds.	Inches.	Seconds.
30·40	30·33			
28·25	32·25	1·92	2·15	·893
25·70	34·75	2·50	2·55	·980
22·45	37·75	3·00	3·25	·925
19·65	41·50	3·75	2·80	1·339
15·95	45·50	4·00	3·70	1·081

There are here evident indications of the rate of retardation being somewhat greater at low than at comparatively high pressures; but, neglecting these indications, the above numbers give 1·043 second as the average retardation in a six-inch or thirty-seconds fuse for each inch of mercurial pressure removed. This result agrees closely with that obtained by Quartermaster Mitchell, if we except those fuses which he burnt at the greatest altitude; and in reference to which some error must obviously have crept in. The following table shows Mr. Mitchell's results uniformly with those in the last table. The fuses which he employed were fifteen-seconds or three-inch ones, and their times of combustion have therefore been multiplied by two in order to bring them into comparison with the six-inch fuses which were used in the author's experiments:—

Pressure of air in inches of mercury.	Average time of combustion of six-inch fuse.	Increase of time of combustion over last observation.	Reduction of pressure corresponding to increase of time.	Increase of time for each diminution of one-inch pressure.
	Seconds.	Seconds.	Inches.	Seconds.
29·61	28·50			
26·75	31·56	3·06	2·86	1·070
23·95	34·20	2·64	2·80	·943
22·98	36·25	2·05	·97	2·113

Here, omitting the last determination as abnormal, we have the average retardation, in the combustion of a six-inch fuse, for each

diminution of one-inch mercurial pressure, equal to 1.007 second, which coincides almost exactly with the number (1.043) deduced from the author's experiments.

The results of both series of observations may therefore be embodied in the following law:—*The increments in time are proportional to the decrements in pressure.* For all practical purposes the following rule may be adopted:—*Each diminution of one inch of barometrical pressure causes a retardation of one second in a thirty-seconds fuse; or, each diminution of atmospheric pressure to the extent of one mercurial inch increases the time of burning by one-thirtieth.*

This retardation in the burning of time-fuses by the reduction of atmospheric pressure will probably merit the attention of artillery officers. Up to the present moment these fuses have been carefully prepared so as to burn, at Woolwich, a certain number of seconds; but such time of combustion at the sea-level is no longer maintained when the fuses are used in more elevated localities. Even the ordinary fluctuations of the barometer in our latitude must render the time of the combustion of these fuses liable to a variation of about ten per cent. Thus a fuse driven to burn thirty seconds when the barometer stands at 31 inches, would burn thirty-three seconds if the barometer fell to 28 inches. Even the height to which a shell attains in its flight must exert an appreciable influence upon the burning of its time-fuse; to a still greater extent, however, must the time of combustion be affected by the position of the fuse during the flight of the shell. If it precede the shell, the time of burning must obviously be considerably shorter than if it follow in the comparatively vacuous space behind the shell.

The apparently opposite conclusions to which we are led as regards the influence of atmospheric pressure upon the *rate* of combustion, by the experiments upon candles on the one hand and upon time-fuses on the other, are by no means irreconcilable; in fact, an examination into the conditions of combustion in the two cases scarcely leaves room for the expectation of any other result. In the combustion of a candle, the author proves that, at all pressures, there is a sufficient supply of melted combustible matter kept up at the base of the exposed portion of the wick: the capillarity of the latter is not affected by pressure; and as the temperature of the

flame is also proved to remain practically constant, effecting the evaporation of the same amount of combustible matter under all pressures, it follows that the rate of consumption of a candle must be nearly or quite independent of the pressure of the surrounding medium. In the deflagration of time-fuses, the conditions are obviously very different. Here the combustible matter never comes into contact with atmospheric oxygen until it has been ejected from the fuse-case. Unlike the candle, the composition contains within itself the oxygen necessary for combustion, and a certain degree of heat only is necessary to bring about chemical combination. If this heat were applied simultaneously to every part of the fuse composition, the whole would burn almost instantaneously. Under ordinary circumstances, however, the fuse burns only at a disk perpendicular to its axis; and the time occupied in its deflagration necessarily depends upon the rapidity with which each successive layer of composition is heated to the temperature at which chemical combination takes place. This heat, necessary to deflagration, is evidently derived from the products of the combustion of the immediately preceding layer of composition; and the amount of heat thus communicated to the next unburnt layer must depend, in great measure, upon the number of particles of these heated products which come into contact with that layer. Now, as a large proportion of these products are gaseous, it follows that, if the pressure of the surrounding medium be reduced, the number of ignited gaseous particles in contact at any one moment with the still-unignited disk of composition will also be diminished. Hence the slower rate of deflagration in rarefied air.

With regard to the effect of atmospheric pressure upon the light of gas-flames, the author thus expresses the conclusion arrived at:—*Of 100 units of light emitted by a gas flame burning in air at a pressure of 30 inches of mercury, 5.1 units are extinguished by each reduction of one mercurial inch of atmospheric pressure. Hence the decrease in illuminating power is directly proportional to the decrease in atmospheric pressure.*

This law is also proved to apply to gas, the illuminating power of which has been doubled by naphthalization; and consequently it may be regarded as applying to all flames in which hydrocarbons are the source of light.

The investigation has also been extended to the effect of com-

pressed atmospheres upon the light of combustion. Great difficulties were experienced in this branch of the inquiry, as gas could not be used, and recourse must therefore be had to other combustibles, which, as already pointed out, are liable to certain irregularities. Owing to these and other difficulties, satisfactory determinations could only be made between one and two atmospheres. In these determinations, the lamp which replaced the experimental gas flame was supplied with amylic alcohol—a liquid which, whilst affording an appreciable amount of light under one-atmosphere pressure, was found to burn under two atmospheres without smoke, although at a somewhat higher pressure it began to evolve unconsumed carbon. The results obtained approximate closely to those indicated by the above law, deduced from the corresponding determinations in rarefied atmospheres, as will be seen from the following table, in which, the mean of eleven observations is given under each experiment; the column headed “calculated” containing the numbers deduced from the rate of variation of luminosity in rarefied air:—

Pressure.	Illuminating Power.	
	Observed.	Calculated.
1 Atmosphere . . . . .	100	100
2 Atmospheres first. . . . .	263·7	253
2 Atmospheres second. . . . .	261·3	253

Further determinations, in which the illuminating power at three- and four-atmospheres pressure was compared, yielded results differing widely from this law, and indicating a much more rapid increase of light; but as the liability to errors increases greatly at these higher pressures, little confidence is placed in the numbers. The lamp was fed with a mixture of five parts of vinic alcohol and one part of amylic alcohol; it had no appreciable illuminating effect under ordinary atmospheric pressure:—

Pressure.	Illuminating Power.	
	Observed.	Calculated.
3 Atmospheres . . . . .	406	406
4 Atmospheres . . . . .	959	559

In tracing the cause of this variation of light under different atmospheric pressures, the author calls attention to the conditions upon which the light of ordinary flames depends. He shows that it is derived almost exclusively from the separation of carbon particles within the flame, and that it is increased by the augmentation of the amount of carbon thus precipitated, and by an increased temperature in the flame; whilst it is diminished by the separation of less carbon and by a reduction of temperature. The temperature of flame is not materially altered by the rarefaction of the supporting medium; and hence the loss of light cannot arise from a reduction of temperature. On the other hand, the separation of carbon particles is greatly augmented by increased pressure; thus candles evolve much smoke when burnt under a pressure of two atmospheres; whilst even a small alcohol flame, which burns with a pure blue light at ordinary pressures, becomes highly luminous in air four times compressed. Flames which smoke at ordinary pressures become smokeless in rarefied air, and undergo more complete combustion. Whilst, therefore, the light of flames is due to the separation of carbon particles, the latter owe their momentary existence to the absence of sufficient oxygen for their combustion; consequently any influence which causes the more rapid interpenetration of the flame gases and exterior air, must reduce the amount of precipitated carbon, and consequently also the luminosity of the flame. Rarefaction exercises precisely such an influence by increasing, as it is well known to do, the mobility of the gaseous particles, and thus causing the access of a larger amount of oxygen to the region of the flame where precipitated carbon produces luminosity.

An analysis of the gases evolved from a candle flame, burning under a pressure of only eight mercurial inches, proved that there was perfect combustion even at this low pressure.

In conclusion, the influence of atmospheric pressure upon the phenomena of combustion may be thus summed up.

1. The rate of burning of candles and other similar combustibles, whose flames depend upon the volatilization and ignition of combustible matter in contact with atmospheric air, is not perceptibly affected by the pressure of the supporting medium.

2. The rate of burning of self-supporting combustibles, like time-fuses, depends upon the rapidity of fusion of the combustible com-

position, which rapidity of fusion is diminished by the more rapid removal of the heated gases from the surface of the composition. Hence the rate of burning of combustibles of this class depends upon the pressure of the medium in which they are consumed. In the case of time-fuses, the increments in the time of burning are proportional to the decrements in the pressure of the surrounding medium.

3. The luminosity of ordinary flames depends upon the pressure of the supporting medium ; and, between certain limits, the decrease in illuminating power is directly proportional to the decrease in atmospheric pressure.

4. The variation in the illuminating power of flame by alterations in the pressure of the supporting medium depends chiefly, if not entirely, upon the ready access of atmospheric oxygen to, or its comparative exclusion from, the interior of the flame.

5. Down to a certain minimum limit, the more rarefied the atmosphere in which flame burns, the more complete is its combustion.

## XVI. "On the Anatomy and Physiology of the Spongiadæ."

Part II. By J. S. BOWERBANK, LL.D., F.R.S., F.L.S., &c. Received June 17, 1861.

(Abstract.)

This paper is a continuation of the first division of the subject published in the Phil. Trans. for 1858.

In the second part of this division the author treats of the keratode or horny substance of the skeleton, as regards both its physical and chemical characters, with a view of establishing the animal nature of that substance.

In the third part the membranous tissues are described under two heads :—

1st. Simple membranous tissues analogous to those of the basement membranes of the higher classes of animals ; and

2nd. Compound membranous tissues. These structures consist of simple membranous tissue combined with primitive fibrous tissue. Their most simple forms exist in the membranes lining the interstitial cavities of the sponge and in the dermal membrane.

In the fourth part the fibrous tissues are described as consisting of three principal divisions.

1st. Primitive fibrous tissue. These structures are exceedingly minute, and form an important element in the construction of the compound membranous tissues of the animal.

2nd division. The fibres of the skeleton are described under the following heads :—

1st. Solid simple keratose fibre.

2nd. Spiculated keratose fibre, in which the keratode is the primary element.

3rd. Multispiculate keratose fibre ; the spicula being the primary element, and the keratode the secondary or cementing medium.

4th. Inequi-spiculated keratose fibre. Consisting of skeleton fibres constructed of numerous spicula irregularly dispersed, but congregated into a round or oval massive fibre.

5th. Simple fistulose fibre. A keratose fibre having a continuous simple central canal.

6th. Compound fistulose fibre. A keratose fibre with a continuous central canal, from which secondary small canals branch at about right angles to the primary one.

7th. Regular arenated keratose fibre. Constructed of solid cylindrical fibres, in the centre of which there is a series of grains of sand or other extraneous matters.

8th. Irregular arenated keratose fibre. Consisting of grains of sand or other extraneous matters cemented together into a continuous cylindrical fibre.

In the third division the siliceous fibrous tissues are described as solid cylindrical structures, similar in form to the solid keratose fibres of the second division, but consisting of pure silex in place of keratode.

The fifth part contains descriptions of the cellular structures.

The sixth and last part of this division treats of the physical characters of the sarcode or semi-gelatinoid substance lining the interior cavities of sponges.

## PART II.—*Organization and Physiology.*

The author treats this portion of his subject under the following heads :—



1st. The skeleton. Its general structure and component parts. Under this head the physiological purposes of the various forms of spicula, treated of in the first part of the paper, are described, and their peculiar offices in the sponge pointed out.

2nd. The sarcodous system is considered by the author as the homologue of the mucous lining of the stomach and intestines of the higher tribes of animals, and probably as the equivalent of the nervous system also.

3rd. The interstitial canals are considered as the equivalents of the stomach and alimentary canals of the higher animals.

4th. The intermarginal cavities, situated immediately beneath the surface, and receiving the incurrent streams from the pores, are believed by the author to be the organs for the secretion of the vital fluids of the animal.

5th. The dermal membrane, enveloping the whole of the sponge, and in which the inhalant and exhalant orifices of the animal are situated.

6th. The pores or inhalant orifices. These organs are not permanent; *i. e.* they are opened and closed at the will of the animal, and when once closed seldom occur again in precisely the same spot.

7th. The oscula or excurrent orifices, usually permanent organs, and capable of being opened or closed in accordance with the necessities of the animal.

8th. Inhalation and exhalation. Two modes of these operations are described; one as occasional or intermittent, but very powerful for the imbibition of nutriment; the other gentle and continuous for the purposes of the aëration of the vital fluids, and for the ejection of digested matters.

9th. Nutrition. The modes of imbibition and periods of digestion are treated of, and the author describes a series of contrivances by which some sponges are in possession of peculiar organs which enable them to prey upon annelids or other soft creatures that may crawl over their surface or intrude within their cavities or canals.

10th. Cilia and ciliary action. The accounts of the cilia of the gemmules or ova, as described by Dr. Grant and other writers, are referred to by the author; and the same organs *in situ* in *Grantia compressa* are pointed out as the powers on which inhalation and exhalation are dependent.

11th. Reproduction, gemmules, &c. Under this head the ovaries, ova, and gemmular modes of propagation, are described under five heads :—

- 1st. Ova without an ovarium.
- 2nd. Ova generated within ovaria.
- 3rd. Gemmules secreted within the sponge.
- 4th. Gemmules produced externally.
- 5th. By spontaneous division of the sarcode.

The remaining portions of the paper, consisting of observations on the generic characters, the specific characters, and on the mode of examination, will form the subject of a future communication.

## XVII. "Further Observations upon the Nebulæ, with practical details relating to the Construction of Large Telescopes."

By the EARL OF ROSSE, F.R.S. Received June 5, 1861.

(Abstract.)

In this paper the processes which were found best to answer in casting specula of 6-feet aperture have been described at some length ; and the precautions which were taken to prevent accidents during the progressive stages of manufacture.

Some information, which may perhaps be useful, has also been gathered from the memoranda kept on each occasion when the specula were ground and polished ; and the results of a long experience in the different manipulations have been put together in a practical form, so as best to enable the civil engineer to undertake the construction of large telescopes as a matter of business.

A few hints have been given on figuring and repolishing large surfaces, which the astronomer may find useful should his services be required in distant countries.

An attempt has been made to define the extreme limits to which telescopic vision may be pushed in this country, in answer to the question whether instruments of greater dimensions might not be advantageously constructed.

The peculiarities in the mounting of the large telescope are slightly noticed, and their advantages and disadvantages explained, as they have been brought out by experience.

This is followed by a selection from the observations made during

a period of six years, accompanied by drawings of the more remarkable objects.

The principal results seem to be a large addition to the list of nebulae with curved or spiral branches, and many new double and multiple nebulae. A variety of objects have been also pointed out upon which the labour of a careful scrutiny will probably be amply repaid, with a similar instrument, even in this climate. A still larger number have been marked off, which to save time may be passed by, unless some new views on Cosmogony should suggest sufficient motives for reobserving them. A record has been made, which, to some extent, will be available hereafter for comparison with the heavens, and a few cases of suspected change have been noticed, where the evidence, however, is by no means conclusive.

XVIII. "Observations on the Posterior Lobes of the Cerebrum of the *Quadrumana*, with a Description of the Brain of a *Galago*." By WILLIAM H. FLOWER, Esq., Demonstrator of Anatomy at the Middlesex Hospital. Communicated by Dr. SHARPEY, Sec. R.S. Received June 20, 1861.

(Abstract.)

After referring to the present state of our knowledge upon the subject, and especially to the descriptions recently given of the "posterior cornu of the lateral ventricle" and "hippocampus minor" in the *Orang-Utang* by Dr. Rolleston, in the Chimpanzee by Mr. Marshall, and in *Ateles* by Mr. Huxley, and the statements of M. Gratiolet, that the posterior cornu of the lateral ventricle or "*cavité ancyroïde*" obtains an enormous development in Monkeys, the author proceeds to detail his own observations (which are illustrated by drawings and photographic representations) upon the condition of these parts in the brains of animals belonging to the three families of the order *Quadrumana*.

#### Family 1. CATARRHINA.

*Orang-Utang* (*Pithecus satyrus*).—An account is given of the examination of the brain of a young female of this species, preserved in the Middlesex Hospital Museum. The posterior lobes of the cerebral hemispheres were so far developed that they completely covered the cerebellum, although not prolonged backwards to quite

so great an extent as in the human brain. To examine the interior, the right hemisphere was removed to the level of the inferior surface of the corpus callosum, and then further portions were carefully dissected away, so as to expose the lateral ventricle, with its three cornua and their contained structures. It is to be observed that brains which have been long in spirit are in some respects not so well adapted for dissection as when in a recent condition, as the distinction in appearance between the white and grey substance is lost, and the contiguous walls of cavities, which in the natural state would have fallen apart, have now become hardened in such close contact, that their real nature may readily be overlooked. In this way only can the statements of Tiedemann as to the absence or rudimentary condition of the posterior cornu in the *Simiæ* be accounted for. However, the brain of this Orang is sufficiently well preserved to show that its ventricular cavity presents almost the exact counterpart of that in the human subject. The posterior cornu extends quite as far backwards as in an average example in man, its apex being but  $\frac{3}{8}$  of an inch ( $=\frac{1}{4}$  of the entire length of the hemisphere) from the occipital extremity of the hemisphere; and its direction well answers to the description "backwards, outwards, and then inwards." Upon the inner wall of the cavity is the hippocampus minor, which will bear comparison with a very well-developed specimen of this structure as met with in the human brain, where, as is well known, it is subject to great variations in size and form. Its length is  $\frac{3}{8}$  inch, its breadth at the base  $\frac{1}{16}$  inch. The portion of the wall of the ventricle situated opposite the junction of the descending and posterior cornua (called *eminentia collateralis* or *pes accessorius*) corresponds in configuration and relative size to the similar part in the brain of man. The hippocampus major has no distinct digital marks; these are, however, indicated by a nodulated appearance in the expanded termination. The remaining portion of the ventricle presented nothing requiring special remark. In order to verify these observations by an examination of the corresponding parts of the opposite side, the upper part of the left hemisphere was also removed, and a very good view obtained of the ventricle, with its posterior cornu. It was precisely similar to that just described, the two sides exhibiting in their internal structure a perfectly symmetrical appearance.

*Cercopithecus*.—Four examples of this genus which have lately died at the Gardens of the Zoological Society were examined while in a recent condition. The first was *C. pygerythrus* (the Vervet Monkey). In order that the brain might be examined *in situ*, the right side of the cranium was removed in the following way. First, a longitudinal incision was made with the saw a quarter of an inch to the right of the middle line, from the supraorbital ridge to the foramen magnum; then with the bone forceps the whole of the cranial wall thus marked out, was carefully cut away down to the base, as were also the right halves of the three upper cervical vertebræ. The dura mater being then removed, a photograph was taken, in which the relative position of the different parts of the brain are well seen. The posterior lobes of the cerebrum project to the extent of  $\frac{1}{4}$  inch beyond the cerebellum, covering it more completely than in the Orang, and nearly, if not quite, as much so as in man. The upper part of the remaining portion of the calvarium was now removed, a section made across the hemispheres at the level of the lower surface of the corpus callosum, and the lateral ventricles opened out on both sides. Although in general form and in the arrangement of the structures composing their walls these cavities present a great resemblance to those of the human brain, one remarkable peculiarity immediately strikes the observer, viz. the great development of the posterior cornu, with the contained hippocampus minor. It extends from the commencement of the descending cornu to near the apex of the well-developed posterior lobe, is of considerable vertical depth, being curled round the voluminous projection of the hippocampus minor, and is directed at first somewhat outwards and backwards, then directly backwards, and finally takes a considerable sweep towards the middle line,—the characteristic form which has obtained for this part in man the name of “digital cavity.” The hippocampus minor is formed, as in the human brain, by the deep involution of a layer of superficial grey cerebral matter, covered internally by a layer of white substance, which is so thin that the surface of this prominence had a darker look than the other parts of the ventricular walls. It differs from a typical example of the corresponding part in the human subject in its great relative size, both as to length and as to the extent to which it projects into the ventricle. The hippocampal sulcus (well marked on the inner

surface of the posterior lobe of all apes) is not only very deep, but has concealed within it a convolution of considerable size, in the form of a longitudinal eminence attached to the floor of the fissure. The eminentia collateralis is prominent. The hippocampus major is smooth upon the surface. The anterior cornu is of the same form and extent as in the human brain.

In the brains of *C. sabæus*, *C. mona*, and *C. ruber* a similar disposition of these parts was found. In the last named, the posterior lobe of the cerebrum is even more prominent, and the hippocampus minor of still greater size, as it tapers less towards its termination; in fact this eminence is here actually larger than the hippocampus major, to which its true relation can be better studied in these apes than in man.

*Macacus*.—In a monkey of this genus (*M. erythræus*) the posterior cornu and hippocampus minor were observed to obtain almost as large a development.

#### Family 2. PLATYRRHINA.

In *Cebus apella*, the ventricular cavity resembles in all essential particulars that of *Cercopithecus*. There is the same extent of posterior cornu and the same complex arrangement of anfractuosités, producing the very protuberant hippocampus minor.

It is among the members of this family (e. g. *Saimiris*) that the projection backwards of the posterior lobes of the cerebrum attains its greatest extent.

#### Family 3. STREPSIRHINA.

The cerebral anatomy of the Lemurs is still imperfectly known; therefore a detailed description is given in the paper of the brain of a species of *Galago*, the most important part of which is the following note upon its internal structure:—"A horizontal section of both hemispheres was made at the level of the corpus callosum, and the lateral ventricles laid open. A broad and very distinct posterior cornu extends backwards almost to the extremity of the hemisphere, occupying nearly the whole of the posterior lobe. Its floor and inner wall are raised into a prominence, having distinctly the characters of the hippocampus minor as found in man and the higher Quadrumana, and corresponding with the bottom of the sulcus before noted on the under surface of the lobe. The form of this eminence is somewhat triangular, the apex being directed backwards; but the surface

is convex, both from above downwards, and in the antero-posterior direction, so that the axis of the cavity that contains it, though directed generally backwards, has first an outward inclination, and finally turns somewhat inwards. The anterior or broad end of the eminence is concave, being adapted to the curved posterior margin of the hippocampus major, from which it is separated by a deep groove. The length of the hippocampus minor is  $\frac{1}{4}$  of an inch, its breadth at the base almost as much. The outer wall of the ventricle has a distinct projection into the angle between the hippocampi, nearly corresponding with the 'eminencia collateralis' of the human brain. On comparing the posterior lobe and hippocampus minor in *Galago* with the same parts in the true apes (e. g. *Cerco-pithecus*), it is seen that though the anterior part is proportionally as broad, the length is considerably diminished; the portion that is wanting being equivalent to that part which, in the apes, covers the posterior third of the cerebellum, and projects beyond it."

As none of the authors who have written upon the brains of the Lemuridæ describe a hippocampus minor, as Vrolik expressly states that it is absent in *Stenops*, and as Burmeister alone assigns a posterior cornu to the ventricle (in *Tarsius*), it seemed desirable, after the results of the observation of these parts in *Galago*, to re-examine the brain of some other members of the family. Two specimens in spirit of *Loris* (*Stenops*) *Bengalensis*, placed at the author's disposal for this purpose by Dr. Grant, afforded distinct evidence of the existence of a well-developed posterior cornu and hippocampus minor, though unfortunately in neither instance were the brains in sufficiently good preservation to allow of a satisfactory description or figure of the parts being made.

*Galago* and *Stenops* being generally considered as not very elevated forms in the Lemurine family, we can have but little doubt as to the presence of the posterior cornu and hippocampus minor throughout the different members of the group, and hence a most important character is supplied for determining the affinities of these interesting animals. It indicates as decisively their position among the Quadrumana, as it separates them completely from the Insectivora, in which order some naturalists have placed them.

Many links are still wanting in the chain of evidence required to determine the true history and classificatory value of the posterior

horn of the lateral ventricle, and the peculiar disposition of cerebral substance constituting the hippocampus minor, but the conditions in which they have been found at so many distinct points of the series, appear to lead almost irresistibly to the following conclusions:—

1. That these parts, so far from being (as has been stated by some anatomists) peculiar to the human brain, are common to man and the whole of the *Quadrumania*, including even the lowest forms.

2. That they attain their maximum of development in species which do not belong to either extremity of the series.

3. That in the lower forms their diminution takes place chiefly in the antero-posterior direction, corresponding with the reduced length of the posterior cerebral lobes, the greater part of which is occupied by them.

4. That in the higher forms they are narrower in proportion to their length, and bear a smaller ratio to the surrounding mass of cerebral substance.

5. That the extreme of the last condition is met with in man, where these parts are also characterized by their variability in size and form, want of symmetry on the two sides, and frequent rudimentary condition, or even entire absence.

**XIX. "On Liquid Transpiration in relation to Chemical Composition."** By THOMAS GRAHAM, Esq., V.P.R.S., Master of the Mint. Received June 20, 1861.

(Abstract.)

In accordance with the analogy of the transpiration of gases, the passage of liquids under pressure through a capillary tube is spoken of as liquid transpiration. The subject owes the development which it has already attained chiefly to the investigations of M. Poiseuille. The precision of the mode of experimenting pursued by that physicist has been remarked on by every one who has engaged in the inquiry. The same method was accordingly adopted with little variation in the present investigation.

The isolated observation made by M. Poiseuille, that alcohol diluted to different degrees is most retarded in passing through a capillary tube at that degree of dilution where the greatest condensation of the mixed liquids occurs, was understood by the author as indicating that the definite hydrate of alcohol containing six equiva-



lents of water (or three equivalents of water with the formula of alcohol taken as  $C, H_2, O_2$ ), was the most retarded in transpiration. The rate of transpiration appeared here to depend upon chemical composition, and to afford an indication of it. A new physical property may thus become available, like the boiling-point and others, for fixing the chemical constitution of substances. The same feature was recognized in methylic alcohol, although the six-hydrate here is not remarkable for condensation of volume; and the inquiry was then extended to the hydrated acids, and to other substances. The results appear to establish the existence of a relation between transpirability and composition.

The time of passage of equal volumes of different liquids under the same pressure and at the same temperature, may be termed their transpiration times, and be referred to the time of water as unity. The transpiration of nitric acid,  $NHO_3$ , with and without water, at  $20^\circ C$ . was as follows:—

Water added to 100 Nitric acid ( $NHO_3$ ).	Transpiration time (water = 1).
0 .....	0.9899
25.47 .....	1.9885
28.56 (2 equivalents) .....	2.0258
30 .....	2.0459
40 .....	2.0833
42.85 (3 equivalents) .....	2.1034 (the maximum).
45 .....	2.0977
50 .....	2.0919
55 .....	2.0632
57.12 (4 equivalents) .....	2.0459
60 .....	2.0387
70 .....	1.9626
80 .....	1.8994
90 .....	1.8261
100 .....	1.7040
200 .. ..	1.3563

The transpiration time rises with successive additions of water, till the proportion corresponding to three equivalents is reached, when the time is 2.1034, and has attained its maximum. Diluted beyond this point the nitric acid begins to pass more freely, and the transpiration time approaches again to that of water. The hydrate named,  $NHO_3 + 3HO$ , having sp. gr. 1.4, possesses the highest boiling-point, and the character of definite composition. It is what

I have elsewhere spoken of as the "constitutional" hydrate of nitric acid.

In acetic acid, the constitutional hydrate,  $C_2H_4O_4 + 2HO$ , is indicated by transpiration with equal precision. The transpiration time rises from 1.2801, the time of the basic hydrate,  $C_2H_4O_6$ , to 2.7040, the time of the hydrate first referred to; falling again afterwards as the water is increased. Butyric and valerianic acids present the same character, although slightly modified. Formic acid, on the other hand, departs entirely from the acetic type in transpirability, as it does in the density of its combinations with water, and in its indisposition to form basic salts. It is curious that liquid formic acid, although an acetic acid by derivation, should more resemble hydrochloric acid in physical characters.

The transpiration time of sulphuric acid is 21.6514, a high number, as might be expected from the viscosity of the liquid. But the time rose still further with the addition of water, till 17.5 parts of water were added to 100 of oil of vitriol, when the number was 23.7706. The proportion of water stated approaches closely to 18.36 parts, which represent 1 equivalent. Here again a well-known constitutional hydrate is indicated,  $SHO_4 + HO$ .

In hydrochloric acid the only sensible retardation observed was with the hydrate represented by  $HCl + 12HO$ . This is the hydrate which possesses least volatility at the low temperature of the experiment ( $20^\circ C.$ ).

It was supposed that glycerine, as a triatomic alcohol, might affect combination with water in the proportion  $C_3H_8O_4 + 18HO$ . But no such compound was indicated by transpiration of the aqueous solutions of glycerine.

The transpiration of pure acetone is remarkably rapid, and is greatly retarded by the addition of water. The time rises from 0.401, that of anhydrous acetone, to 1.604, the time of the twelve-hydrate, taking the equivalent of acetone as  $C_3H_4O_2$ , or of the six-hydrate with the equivalent  $C_3H_4O$ .

The transpiration times and boiling-points of three alcohols are as follows :—

	Transpiration time.	Boiling-point.
Methylic alcohol . . . . .	0.630 . . . . .	66 C.
Vinic alcohol . . . . .	1.195 . . . . .	78.5
Amylic alcohol . . . . .	3.649 . . . . .	132

Of four others :—

	Transpiration time.	Boiling-point.
Formic ether .....	0·511 .....	55·5
Acetic ether .....	0·553 .....	74·0
Butyric ether.....	0·750 .....	
Valerianic ether.....	0·827 .....	133·5

Judging from these last observations, the order of succession of individual substances in any natural series will be indicated by the individual transpirability of these substances, as clearly as it is by their comparative volatility. Transpiration and boiling-point observations may come thus to claim an equal interest. In carrying out the inquiry, it will probably be found advantageous to transpire the liquids at a fixed temperature which is somewhat elevated. A large number of substances are liquid at 100° C., of which the transpiration times could easily be obtained. Slow transpiration and low volatility appear to go together, and both to be connected in a general way with a heavy molecule. So also the annexation of constitutional water to the hydrated acids and alcohols appears to impede the transpiration of these substances.

XX. "Electro-Physiological Researches.—Eleventh Series. On the Secondary Electromotor Power of Nerves, and its Application to the Explanation of certain Electro-Physiological Phenomena." By Professor C. MATTEUCCI. Communicated by General SABINE, Treas. and V.P.R.S. Received June 2, 1861.

(Abstract.)

The object of this paper is to show by experiment that when a nerve is traversed by an electric current, it acquires in all its points a secondary electromotor power, and consequently becomes capable of producing in a conducting homogeneous circuit, whose extremities touch any two points of that nerve, an electric current in an opposite direction to that of the original current. This result is independent of the vital properties of the nerves, but is affected in greater or less degree by their physical condition. A similar effect indeed is produced by the passage of an electric current in all porous substances

imbibed with a conducting liquid, and the phenomenon has been studied in its generality by other physicists ; but the purpose of the present paper is to determine the conditions of the secondary electromotor power of nerves, in order to make a due application of these conditions to the explanation of the phenomena exhibited by nerves on the opening of a voltaic circuit which has traversed them.

Having explained the object of his memoir, the author, before giving an account of his experiments, proceeds to describe certain improvements he has lately made in the instruments he employs for electrophysiological researches, whereby he is able more easily and effectually to avoid the risk of disturbing currents liable to be produced in the apparatus itself.

The fundamental experiment on which the main position of the memoir rests is performed as follows :—The sciatic nerve taken from a frog, a fowl, or some other recently killed animal, is used for the purpose. The operator first assures himself that no sign of current is manifested on simultaneously touching with the galvanometer two points of the nerve equidistant from its cut extremities. The disturbing effect of the electric current naturally generated in nerves may also be eliminated by placing two nerves, or two portions of nerve, in such relative position that their natural currents shall be opposed in direction and mutually neutralize their effects on the galvanometer. To the nerve or piece of nerve thus tested are applied, at some distance from its extremities, the electrodes of a pile of eight or ten elements, and the exciting or pile-current is allowed for a short time to pass along the included part of the nerve. When the nerve is now put in communication with the galvanometer, the needle deviates, and indicates that the nerve is traversed, in the portion which had been included between the electrodes of the pile, by a current the direction of which is opposite to that of the current of the pile, and which lasts for a certain time. Signs of secondary current are also obtained by applying the galvanometer to the parts of the nerve which have not been traversed by the pile-current, that is, the end-parts between the extremities of the nerve and the points touched by the electrodes of the pile ; but the secondary currents in these end-portions of the nerve are in the same direction as the pile-current, and therefore opposed to that of the secondary current developed in the part included between the electrodes and traversed

by the pile-current. It is further observed, that of the two end-currents the one adjacent to the point of application of the negative electrode is stronger than the other.

It is to be noted that the secondary current endures for some time after the cessation of the exciting current; hence it is evidently not caused by induction. The author thus explains its production:—At the points of a nerve which have been acted on by the electrodes of a pile the products of electrolysation are accumulated, and thence spread through the tissue more or less, according to differences of its structure and chemical disposition; conditions, persistent for a time, are thus established for generating a current when the circuit is completed between two different points of the nerve. The same thing happens when a strip of paper or flannel, moistened with a weak saline solution, is first subjected to the current of a pile and then tested with the galvanometer, or if such a strip is so tested after having been simply wetted at one part with acid and at another with alkaline solution, to represent the effect of electrolysation by an exciting current; and in either case the direction of the secondary current in the moistened strip, both in the part included between the points of application of the electrodes and in the excluded parts at the ends, corresponds with that in the nerve.

The experiment succeeds perfectly in the entire nerve of a living animal, such as the sciatic of a rabbit or a fowl. But the result is independent of the vital condition of the nerve, for the effect is found to be equally great four days after death as at the moment an animal is killed.

The author next gives the results of experiments made to determine the influence of various physical and chemical conditions on the secondary electromotor power of nerves. The method he generally followed was by comparing the secondary currents caused by the same pile-current in a natural and in an altered nerve. He had previously ascertained that when the same pile-current is passed through two nerves at once they are equally affected, and give when placed in opposition no differential deflection of the galvanometer, although singly each might give a secondary current of  $40^{\circ}$  or  $50^{\circ}$ . Now by sending the same pile-current simultaneously through two nerves or portions of nerve, either before or after the one of them has been subjected to the particular conditions to be tried, and finally placing the two in opposition in connexion with the galvanometer, the occurrence

or absence of differential deflection of the needle will show whether any or what degree of effect has been produced on the secondary electromotor power of the altered nerve. In this way the author found that both the cold of a refrigerating mixture and a heat of from  $50^{\circ}$  to  $60^{\circ}$  Cent. caused a great diminution of the secondary current in the nerves of a fowl. It is greatly weakened also by crushing the nerve, or by keeping the nerve long immersed in distilled water; immersion for a few seconds has no appreciable effect. Immersion for a few minutes in alcohol, or in a solution of potash of  $\frac{1}{3000}$  strength, entirely extinguishes the secondary current. Two nerves subjected to the same exciting current, the one fifteen or twenty minutes after the other, show no difference in their secondary currents. If an exciting current is passed through a nerve, first in one direction and then for an equal time in the opposite direction, the secondary current produced is weaker than if the pile-current had been in one direction only. The secondary current increases, within certain limits, with the duration, and also with the intensity of the pile-current. Neither the size nor the number of the nerves (if united by superposition) exercises any influence. Four similar nerves were subjected to the same pile-current; three of these nerves *superposed* were then placed in opposition to one as a differential pile, but no differential current was produced. Portions of nerve of equal length from a frog, a lamb, and a fowl, were subjected to the same pile-current, and when afterwards opposed successively, the one to the other, gave no differential current; yet singly each gave a secondary current of  $40^{\circ}$  or  $50^{\circ}$ . A decided effect, however, is produced by the length of the nerve; when pieces of different lengths were compared, a strong differential current was constantly found to correspond with the longest portion of nerve; the author remarks that this result cannot be understood, unless we admit that the secondary electromotor power, which is originally greatest in contact with the electrodes, extends successively to all the parts of a nerve traversed by the pile-current.

The author then comes to the fact which he conceives to be of greatest importance, in the application of the doctrine of the secondary electromotor power of nerves to the explanation of certain electrophysiological phenomena. It is expressed in the following proposition:—

*"The secondary electromotor power of a nerve is not equal in all parts of the nerve, being much stronger in the portion of the nerve near the positive electrode, than in that near the negative electrode; and this difference is greater in a nerve which has been traversed by the current in the direction contrary to that of its ramification, than in a nerve traversed by a current in the same direction as its ramification."*

The unequal intensity of the secondary current and its maximum near the position of the positive electrode, is shown by cutting an electrolysed nerve into two or more equal lengths, which are then severally opposed to each other in connexion with the galvanometer, and the greatest differential current is found to be produced by the part near the situation of the positive electrode. Again, the greater effect of a pile-current passed inversely to the direction of the ramification of the nerve, is shown by opposing two nerves which have been traversed in different directions by the same current; or by dividing a nerve and opposing its two equal halves after they have been traversed in opposite directions by the same current.

The application of these facts respecting the secondary current in nerves to the explanation of the phenomena which take place in nerves on the opening of a circuit, is treated of in the concluding part of the paper, which we give with slight abridgement in the author's own words:—

"The object of these researches," he observes, "was not to study the production of secondary electromotor power in nerves rather than in other porous and humid bodies of various structure and chemical composition. Under this point of view it is evident that the phenomenon is complex and its analysis difficult. In the present state of science, therefore, we are unable to account for the differences presented by a nerve in its different points, according to their proximity to one pole or the other, and the direction in which the nerve is traversed by the current. It is possible that similar differences will present themselves in other bodies not organized nor taken from living animals. It is sufficient for my present object to have proved that the secondary electromotor power of a nerve requires for its development the integrity of structure of the nerve itself, not, however, the excitability of the living animal; and to have determined rigorously the differences of this power which have led me to ground

the explanation of the electro-physiological phenomena which take place on the opening of the circuit, on a fundamental physical fact.

"If in a frog prepared in the usual way for electrical experiments a continuous current is passed up one hind limb and down the other, for 15, 20, or 30 seconds, according to the force of the current, it is known that the opening of the circuit is accompanied by violent contractions of the limb traversed by the inverse current. These contractions depend, as I showed many years ago\*, on a particular state of the nerve; and in fact the contractions are obtained and continue when the circuit is interrupted by cutting the nerve near the spine, but they are no longer produced if the nerve is cut near its insertion in the muscles of the leg.

"My object in this memoir has been to prove that the particular state of the nerve above described consists in secondary electromotor power, that is, in a well-known physical phenomenon. The course of the secondary current, which is downward or direct in the nerve that has been traversed by the upward or inverse pile-current, explains, according to the well-known laws of electro-physiology, the effects produced by it on that nerve on the opening of the circuit.

"The differences of electromotor power found in various points of the electrolysed nerve, the prevalence of this power in the part of the nerve near the positive electrode, very probably also the different degree of this secondary electromotor power in the various strata which compose the interior and the envelope of the nerve, are circumstances which seem to explain the secondary current which takes place in the nerve at the opening of the circuit, and which is direct and most intense in the nerve which has already been traversed by the inverse current, most intense also in the vicinity of the positive electrode.

"In order, therefore, to explain the phenomena which accompany the opening of a circuit, we must henceforth have recourse to the fact of the development of secondary electromotor power in nerves and the laws according to which it manifests itself."

\* Phil. Trans. 1847, pp. 235, 236.



XXI. "Supplement to Two Papers published in the Philosophical Transactions (1820 and 1825) on the Science connected with Human Mortality." By BENJAMIN GOMPERTZ, Esq., F.R.S. Received June 19, 1861.

(Abstract.)

The first of the two Papers referred to is entitled "A Sketch of an Analysis and Notation applicable to the estimation of the value of Life Contingencies." It is especially devoted to the explanation of a new notation and a new mode of analysis applicable to the subject. The second Paper, entitled "On the Nature of the Function expressive of the Law of Human Mortality, and on a new mode of determining Life Contingencies," treats, in its first part, of the equation  $L_x = d \cdot g^x$ , in which  $L_x$  denotes the number of persons who would be living at the age  $x$ , out of the number of persons who may have been living at some given common previous age, in which  $d, g, q$  may be considered constant quantities through a long series of years, and the equations to give the real rate of mortality during that period within a very satisfactory approach to coincidence, say, for instance, from the age ten to the age sixty; a theorem from whence the number living at an assigned age may be derived, by what the author calls the vital rule of three, from the number living, for instance, at the age twenty, forty, and sixty, given in a table of mortality. But the author observes that though the above formula with suitably determined constant values of  $d, g, q$  appears within a great approach of the observed result, to agree with the tabular data through so long a period, still that actually  $d, g, q$  are not constant quantities, as every different selection from the table of the living to which the formula is applied, for the purpose of determining the three constants contained in it, even if that table throughout were an accurate representation of the law of mortality, which no table based on obtainable data can be expected to be, would give different values of  $d, g, q$ , as they are not really absolutely constant, but slightly variable throughout, because the formula  $L_x = d \cdot g^x$  does not represent perfectly the law of mortality, if  $d, g, q$  are perfectly constant; and the author complains of its misinterpretation in this respect by a subsequent writer, who lays claim to discovery on this subject.

The other part of the Paper of 1825 is devoted to a mode with accompanying tables, to enable a calculator with any given table of mortality to calculate the value of an annuity on any number of joint lives to within any degree of accuracy at pleasure; provided the periods, if long, are divided into parts.

The present supplement, written so many years after the original papers were communicated to the Society, appeared to the author to be called for in order that he might further illustrate the subject, and add his later speculations and improvements. The supplement the author commenced writing about two years ago from collections of his manuscripts, but was prevented by continued ill-health from proceeding with it; but having received an invitation last year from the International Congress to assist in their meritorious labours, and being unable from indisposition to attend the Meeting, as he stated to the Congress, he offered to send them some hints respecting his recent labours, the results of which he intended to present to the Royal Society, if his health should permit him to finish the papers. The hints which he gave, which were honoured by a place in the Reports of the Congress, were, he believes, deemed interesting, but he thinks they were sufficiently separated from the strictly mathematical part of the subject, and also from the most important portions of the result, to allow him to consider the present paper a new work, or sufficiently new, to be thought worthy of presentation to the Royal Society, and especially because since the notice above alluded to was furnished, the author has been able to introduce important improvements. The original formulæ give one uniform law of mortality from birth to the utmost limit of the table; but in the case of the table presented to the Congress, and of the present further improved formulæ, if a comparison be made, for instance, with the Carlisle table from birth to the age of 100, and even from birth to one month, to two months, to three months, to six months, and to one year, where the tables appear to be so irregular, and formed on no law, the result appears to show that a law really exists and is available.

The author shows the vast use and applicability to solutions of all intricate inquiries of life contingencies of his present deductions. He gives a theory, which he believes to be quite new, which he calls special, single, and specially influenced contingencies; a subject which he states he had not lost sight of when officially engaged in

the science of assurance, as he considered that all cases of connected lives, where there might be a connexion of any sort, of influences acting with respect to the probability of co-existence, all questions solved without reference to such influence, were faulty, and might be importantly faulty. He states a striking instance with respect to this observation as follows :—If it be required to assure a sum on the joint lives of A and B, that is, to pay the sum on the first of the death of A and B, whichever may be that first, and if in another case it were required to assure the sum on the death of B in particular, provided he should die in the lifetime of A, it is evident that if A and B are of the same age, and not subject to any influential connexion, that if they are both subject to the same uninfluenced mortality, the price of the first assurance would be exactly double the other. But if the assurance were for a very short time, for instance, in the time a ship was passing from Dover to Calais, and they were both going in the same ship together, the risk of the ship being wrecked would affect them both equally, and the chance of their separate escape would depend on circumstances, for instance, one being a better swimmer than the other ; but there would be a chance that neither of them could escape, and as far as that chance is concerned, the assurance on one of the two, whichever it might be, who died first, the assurance on one in particular, or the assurance on the longest of their lives during that voyage would be all of equal value ; and the method the author has adopted for all uninfluenced contingencies would be available in all cases of influenced, by the introduction of certain formulæ which the author regards as of a very interesting nature.

In addition to the above particulars, the author presents a theory of sickness, which appears to him to be a near approach to actual statements of sickness occurring among Friendly Societies.

The Society then adjourned over the Long Vacation, to Thursday, November 21.

"On an Application of the Theory of Scalar and Clinant Radical Loci." By ALEXANDER J. ELLIS, Esq., B.A., F.C.P.S.  
Communicated by A. CAYLEY, Esq. Received February 20, 1861. Read March 14, 1861\*.

1. The following investigation, which contains a correction and extension of that in Plücker's 'System der Geometrie' (§ 3. art. 64), is a direct application of the theories in the writer's paper "On Scalar and Clinant Algebraical Coordinate Geometry" (Proceedings, vol. x. pp. 415-426), to which reference must be made for an explanation of the notation and terminology.

2. Let  $f(x, y)$  be an algebraical formation (function) of  $x, y$  and of  $n+2m$  dimensions, such that when any scalar value is attributed to  $x, f=0$  has  $n$  scalar (possible) and  $2m$  clinant (imaginary) roots, and let  $\lambda$  be the coefficient of  $y^{n+2m}$ . Then

$$f(x, y) = \lambda(y - f_1 x) \dots (y - f_n x) \times (y - f'_1 x)(y - f'_2 x) \dots \\ (y - f^{(m)}_1 x)(y - f^{(m)}_2 x).$$

3. Now let PQ be a line which, when produced, will cut the curve, whose equations are

$$OM = x \cdot OI + y \cdot OB, \quad f(x, y) = 0,$$

(where  $x$  and  $y$  are scalar, and OI is in the same straight line with OP, and OB is a line equal in length to OI, and parallel to and in the same direction with PQ) in the  $n$  points  $M_1, M_2, \dots M_n$ . Then, if  $OP = x_1 \cdot OI$ , where  $x_1$  is scalar,  $\frac{PM_1}{OB}, \frac{PM_2}{OB}, \dots \frac{PM_n}{OB}$  will represent the  $n$  scalar roots  $f_1 x_1, f_2 x_1, \dots f_n x_1$ . Hence if  $PQ = y_1 \cdot OB$ , the point Q not being necessarily a point in the curve, any one of the factors  $y_1 - f_r x_1$  will be represented by  $\frac{PQ - PM_r}{OB} = \frac{M_r Q}{OB}$ .

Now for the clinant roots†, put  $y = r + i \cdot s$ , where the Roman  $i = \sqrt{-1}$ , and  $r$  and  $s$  are scalar, then must

$$f(x, y) = F_1(x, r, s) + i \cdot s^n \cdot F_2(x, r, s),$$

because there are  $n$  scalar roots for each of which  $s=0$ . For the

\* An abstract of this Paper has already appeared in the "Proceedings," page 141. It is now printed in full by order of the Council.

† Plücker only says that "no imaginary point of intersection must be neglected" (System, p. 45), but he gives no means for taking them into consideration.

clinant roots, then, we have  $F_1=0$ , and  $F_2=0$ ; whence we find, by alternately eliminating  $s$  and  $r$ ,  $F_2(x, r)=0$ , and  $F_1(x, s)=0$ .

Now describe the loci of the points R and S, where

$$OR = x \cdot OI + r \cdot OB, \quad F_2(x, r) = 0,$$

and

$$OS = x \cdot OI + s \cdot OB, \quad F_1(x, s) = 0.$$

Then from the nature of the clinant roots, which will be in pairs of the form  $r_1 \pm i \cdot s_1$ , if we produce PQ to cut these curves, for every point R' in which it cuts the first there will be two points S', S'', in which it cuts the second. If, then, from the point R' we draw  $R'M'_1 = i \cdot PS'_1$  and  $R'M'_2 = i \cdot PS'_2$ , two corresponding clinant roots of the equation will be  $\frac{PM'_1}{OB}$ ,  $\frac{PM'_2}{OB}$ , so that

$$y_1 - f_1^{(r)} x_1 = \frac{PQ - PM_1^{(r)}}{OB} = \frac{M_1^{(r)} Q}{OB},$$

and

$$y_1 - f_2^{(r)} x_1 = \frac{PQ - PM_2^{(r)}}{OB} = \frac{M_2^{(r)} Q}{OB}.$$

See the example and figure in (11).

Hence we shall have for the geometrical representation of any algebraical formation, by (2),

$$f(x_1, y_1) = \lambda \cdot \frac{M_1 Q}{OB} \dots \frac{M_n Q}{OB} \times \frac{M'_1 Q}{OB} \cdot \frac{M'_2 Q}{OB} \dots \frac{M_1^{(m)} Q}{OB} \cdot \frac{M_2^{(m)} Q}{OB}.$$

4. Next suppose that the origin of the coordinates be altered to O', so that O'I' = OI, and O'I', when produced, passes through Q. This will be equivalent to putting  $y \pm b$  instead of  $y$ , and the result will not disturb the coefficient of  $y^{m+2m}$ . Also since one of the radical loci for the scalar values remains unaltered, and the others (which are the axes) are parallel to the former, the points  $M_1, M_2, \dots, M_n$  in which PQ, when produced, will cut the curve, remain the same, and consequently there are the same number of scalar roots as before.

5. But as the terms themselves of the equation  $f(x, y) = 0$  are altered, the radical loci of R and S in (3) will be altered, and as the distances are now measured from Q and not from P, the lines  $QR'_1 + i \cdot QS'_1, QR'_2 + i \cdot QS'_2$  will now be different and  $= Qm'_1, Qm'_2$ . If then O'Q =  $x_2 \cdot O'I'$ , and QP =  $y_2 \cdot BO = -y_2 \cdot OB$ , then

$$f(x_2, -y_2) = \lambda \cdot \frac{M_1 P}{OB} \dots \frac{M_n P}{OB} \times \frac{m'_1 P}{OB} \cdot \frac{m'_2 P}{OB} \dots \frac{m_1^{(m)} P}{OB} \cdot \frac{m_2^{(m)} P}{OB}.$$

Or, replacing OB in each case by BO, we have

$$f(x, y) = \lambda' \cdot \frac{M_1 P}{BO} \dots \frac{M_n P}{BO} \times \frac{m'_1 P}{BO} \dots \frac{m'_n P}{BO};$$

where, since there are  $n+2m$  factors,

$$\lambda' = (-1)^{n+2m} \cdot \lambda.$$

6. But if, instead of simply changing the origin, we change the inclination of PQ and make it parallel to OC, then if OB and OC make the angles  $\beta$  and  $\gamma$  with OI, and OJ be at right angles to OI, the lengths of OI, OJ, OB, OC being the same, and M be any point, we have

$$OM = x \cdot OI + y \cdot OB = (x + y \cos \beta) \cdot OI + y \sin \beta \cdot OJ,$$

and

$$OM = x' \cdot OI + y' \cdot OC = (x' + y' \cos \gamma) \cdot OI + y' \sin \gamma \cdot OJ,$$

which give two simple equations to determine  $x, y$  in terms of  $x', y'$ , so that on substitution  $f(x, y)$  becomes  $\phi(x', y')$ , a formation of the same number of dimensions. But the coefficient of  $y'^{n+2m}$  will be different from that of  $y^{n+2m}$ . Let it be  $\mu$ . The curve for the scalar values of  $x', y'$  in

$$OM = x' \cdot OI + y' \cdot OC, \quad \phi(x', y') = 0$$

will be precisely the same as that in (3); but as PQ, having a different inclination, will cut it in very different places or not at all, the numbers of scalar and clinant roots of  $\phi=0$  may be individually different from, although their combined number will be the same as, those of  $f=0$ . If then there be  $h$  scalar and  $2k$  clinant roots of  $\phi=0$ , where  $h+2k=n+2m$ , and we determine  $N_1, N_2 \dots N'_1, N'_2 \dots$  in the same way as we previously determined  $M_1, M_2 \dots M'_1, M'_2 \dots$  we find

$$f(x, y) = \phi(x, y) = \mu \cdot \frac{N_1 Q}{OC} \dots \frac{N_h Q}{OC} \times \frac{N'_1 Q}{OC} \dots \frac{N'_k Q}{OC}.$$

7. This investigation shows that if there be any number of lines drawn from a point, cutting those radical loci of an equation for scalar and clinant roots which correspond to the inclination of these lines to the axis and to the position of the origin, coefficients  $\lambda, \mu$ , &c. can always be assigned such as to make the product of these ratios each equal to the other; and that these coefficients have only to be multiplied by  $(-1)^{n+2m}$ , in order to give the coefficients corresponding to

such a change of origin as would make the axis pass through the other extremities of the lines.

8. Now let  $O_1O_2 \dots O_nO_1$  be a completely enclosed polygon, the sides of which, when produced, cut the radical loci of a known algebraical equation of  $n$  dimensions in known points. Let the product of the series of ratios formed as above (3), corresponding to lines drawn from the point  $O_r$  in the direction  $O_rO_{r+1}$  (on which last-named line the unit line  $O_rO'_{r+1}$ , corresponding to  $OI$  in (3), is measured), be represented by

$$[O_{r,r+1}M \div O_rO'_{r+1}].$$

And let  $\lambda_{r,r+1}$  be the coefficient necessary to make this product a correct representation of the formation on the left-hand side of the given equation for the values of  $x$  and  $y$ , corresponding to the point  $O_r$  and the direction  $O_rO_{r+1}$ .

Then for the point  $O_{r+1}$  and the direction  $O_{r+1}O_r$  (for which the corresponding unit line is  $O_{r+1}O'_r$ ), we shall have the product of the corresponding series of ratios represented by

$$\lambda_{r+1,r}[O_{r+1,r}M \div O_{r+1}O'_r],$$

where

$$\lambda_{r+1,r} = (-1)^n \cdot \lambda_{r,r+1}.$$

Proceeding then round the polygon, and forming the products for each of the two sides terminating at each point, we have by (7),

$$\begin{aligned} \lambda_{1,n}[O_{1,n}M \div O_1O'_n] &= \lambda_{1,2}[O_{1,2}M \div O_1O'_2] \\ (-1)^n \lambda_{1,2}[O_{2,1}M \div O_2O'_1] &= \lambda_{2,3}[O_{2,3}M \div O_2O'_3], \\ &\dots = \dots \end{aligned}$$

$$\begin{aligned} (-1)^n \lambda_{n-2,n-1}[O_{n-1,n-2}M \div O_{n-1}O'_{n-2}] &= \lambda_{n-1,n}[O_{n-1,n}M \div O_{n-1}O'_n] \\ (-1)^n \lambda_{n-1,n}[O_{n,n-1}M \div O_nO'_{n-1}] &= \lambda_{n,1}[O_{n,1}M \div O_nO'_1]. \end{aligned}$$

Hence multiplying all these  $n$  equations together, and remembering that  $\lambda_{1,n} = (-1)^n \cdot \lambda_{n,1}$ , and that  $(-1)^{nn} = (-1)^n$ , since  $nn$  and  $n$  are both odd or both even, we have

$$[O_{1,n}M \div O_1O'_n] \cdot [O_{n,1}M \div O_nO'_1] \dots [O_{n,n-1}M \div O_nO'_{n-1}] \\ = (-1)^n \cdot [O_{1,2}M \div O_1O'_2] \cdot [O_{2,3}M \div O_2O'_3] \dots [O_{n,1}M \div O_nO'_1].$$

Now since  $O_{r,r+1}M$  and  $O_{r+1,r}M$  are on the same straight line, their ratio is scalar; and since  $O_1O'_n = -1 \cdot O_nO'_1$ , we have, by an

obvious extension of the notation, and by altering the order of the factors,

$$\begin{aligned} & [O_{1,2} M \div O_{2,1} M] \cdot [O_{2,3} M \div O_{3,2} M] \dots [O_{n,1} M \div O_{1,n} M] \\ & = (-1)^n \cdot [O_1 O'_2 \div O_2 O'_1] \cdot [O_2 O'_3 \div O_3 O'_2] \dots [O_n O'_1 \div O_1 O'_n] \\ & = (-1)^n \cdot [(-1)^n \cdot (-1)^n \dots (n \text{ factors}) \dots (-1)^n] \\ & = (-1)^n \cdot (-1)^{n^2} = (-1)^n \cdot (-1)^n = (-1)^{2n} = 1^* \end{aligned}$$

9. Hence if all the roots are scalar, that is, if each side of the polygon cuts the scalar radical locus in as many points as its abstract equation has dimensions, we have the following fundamental proposition of the theory of transversals:—

“If we take the points at the extremities of a side of a polygon, and find the ratios of the distances of these points from the intersections of that side with a curve, and proceed in this manner regularly round the polygon in one direction, the product of all these ratios will be unity.”

10. Thus if the triangle  $O_1 O_2 O_3$  cut a straight line in  $A, B, C$  so that these points lie on  $O_2 O_3, O_1 O_3, O_1 O_2$  respectively, then

$$\frac{O_1 C}{O_2 C} \cdot \frac{O_2 A}{O_3 A} \cdot \frac{O_3 B}{O_1 B} = 1^\dagger.$$

If the same triangle  $O_1 O_2 O_3$  cut a conic in the points  $A_1, A_2$  by  $O_2 O_3$ ;  $B_1, B_2$  by  $O_1 O_3$ ;  $C_1, C_2$  by  $O_1 O_2$  respectively, then

$$\frac{O_1 C_1}{O_2 C_1} \cdot \frac{O_1 C_2}{O_2 C_2} \times \frac{O_2 A_1}{O_3 A_1} \cdot \frac{O_2 A_2}{O_3 A_2} \times \frac{O_3 B_1}{O_1 B_1} \cdot \frac{O_3 B_2}{O_1 B_2} = 1.$$

\* Plücker says,  $= \pm 1$ , and adds, that “it is immediately evident that when the curve is of an even order, the upper sign must be taken; and when it is of an uneven order, either the upper or the lower, according as the number of the angles of the polygon is even or odd” (System, p. 44). Accordingly most of his examples, as shown in (10), give an erroneous result. It must be observed that Plücker does not give the equation in the form above written, but places the product of all the antecedents of the ratios on one side, and the product of all the consequents on the other. As, however, these antecedents and consequents are *directed* lines, such products have no proper meaning. He has in fact considered those directed lines as scalars, but has neglected to assign the directed unit lines with respect to which they are to be considered as scalars. Hence his error.

† Plücker makes the second side of this equation  $= -1$ , which is manifestly erroneous. But if  $O_1 a, O_2 b, O_3 c$  be drawn from  $O_1, O_2, O_3$  through any point  $M$  to  $O_2 O_3, O_1 O_3, O_1 O_2$  respectively, then by considering the intersections of the triangles  $O_1 O_2 a, O_1 O_3 a$  with  $O_2 c$  and  $O_3 b$  respectively, we immediately deduce from the equation in the text,

$$\frac{O_1 c}{O_2 c} \cdot \frac{O_2 a}{O_3 a} \cdot \frac{O_3 b}{O_1 b} = -1.$$



If the triangle touches the conic in  $A, B, C$ , then  $A_1, A_2$  coincide with  $A$ , &c., and the above equation becomes

$$\frac{O_1C}{O_2C} \cdot \frac{O_2A}{O_3A} \cdot \frac{O_3B}{O_1B} = 1 \text{ or } -1.$$

The first case shows that the three points lie on a straight line; the second, that the three lines  $O_1A, O_2B, O_3C$  meet in a point\*.

If the point  $C$  were to lie at an infinite distance from  $O_1$  and  $O_2$ , then since  $O_1C = O_1O_2 + O_2C$ , and  $\frac{O_1C}{O_2C} = 1 + \frac{O_1O_2}{O_2C}$ , where the last term is infinitesimal, we have merely to consider  $\frac{O_1C}{O_2C} = 1$ , or omit it from the product†. Thus if we take as two sides of the triangle the asymptotes of an hyperbola meeting at  $O_1$ , and suppose the third side  $O_2O_3$  to cut the curve in  $A_1$  and  $A_2$ , then since both  $O_1O_2$  and  $O_1O_3$  cut the curve at an infinite distance, the equation reduces to

$$\frac{O_2A_1}{O_3A_1} \cdot \frac{O_3A_2}{O_2A_2} = 1;$$

whence we readily find

$$O_2A_2 = O_3A_2, \text{ or } = -O_2A_1.$$

The first value is impossible, because  $A_2$  lies between  $O_2$  and  $O_3$ ; the second gives the well-known property of the hyperbola‡.

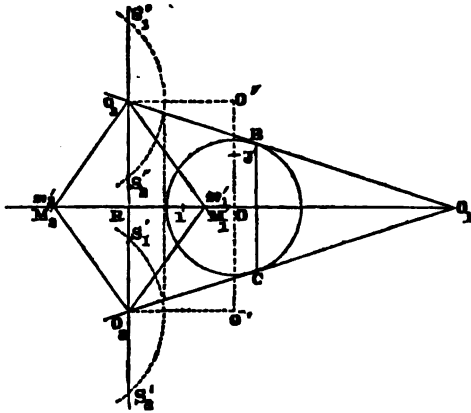
11. As regards the clinant roots, or the intersections of the sides of the polygon with the clinant radical loci, the wording of the proposition (9) requires to be changed to adapt it to the more complicated form of the product; and perhaps it may be considered sufficient to refer to the forms of the products in the final equation of (8),

\* Plücker interchanges the two cases, and makes the positive result show the intersection of the three lines in a point, which is shown to be wrong in the last note. He rejects the first case, "because a conic can only be cut in two points by a straight line" (p. 46). But in this case both the conic and the triangle reduce to a straight line (that is, the points  $O_1, O_2, O_3, A, B, C$  all lie in the same straight line), and the points  $A, B, C$  do not indicate contacts, but double-intersections, which have the same analytical expression, owing to the rejection of infinitesimals in the above calculation of contact.

† Plücker says that "in every such case there must be a fresh change of sign" (p. 46). This apparently arises from his having neglected to particularize the directed unit, or to note the primary relation  $O_1C = O_1O_2 + O_2C$ .

‡ Plücker makes  $O_2A_1 = O_3A_2$  (p. 46); that is, he neglects the relation of direction, which is all-important in such investigations.

and append a single simple example in which the product admits of being easily formed.



Let the triangle  $O_1O_2O_3$  touch a circle  $BC$  in the points  $B$  and  $C$ , and let the side  $O_2O_3$  be perpendicular to the line  $O_1O$  drawn through the centre of the circle,  $O$ , but be wholly exterior to the circle. Take  $O$  as the origin of coordinates, and suppose that the equations to the circle are

$$OP = x \cdot OI + y \cdot OJ, \quad x^2 + y^2 = c^2.$$

Now change the origin to  $O'$ , where  $OO' = \frac{1}{2}O_2O_3 = -b \cdot OJ$ . Then the equations to the circle become

$$O'P = x' \cdot OI + y' \cdot OJ, \quad x'^2 + (y' - b)^2 = c^2.$$

And for the radical loci, when  $y'$  is clinant, we have (3)

$$O'R = x' \cdot OI + r' \cdot OJ, \quad r' - b = 0,$$

that is, the line  $O_1O$ , and

$$O'S = x' \cdot OI + s' \cdot OJ, \quad x'^2 - s'^2 = c^2,$$

that is, the rectangular hyperbola  $S'_1 S'_2$ . The roots will then be

$$\frac{O_2R + i \cdot O_2S'_1}{OJ} = \frac{O_2R + RM'_1}{OJ} = \frac{O_2M'_1}{OJ}$$

and

$$\frac{O_2R + i \cdot O_2S'_2}{OJ} = \frac{O_2R + RM'_2}{OJ} = \frac{O_2M'_2}{OJ},$$

and the factors corresponding to them in the final product in (3) will be

$$\frac{O_2O_3 - O_2M'_1}{OJ} \cdot \frac{O_2O_3 - O_2M'_2}{OJ} = \frac{M'_1O_3}{OJ} \cdot \frac{M'_2O_3}{OJ}.$$

Similarly, by transferring the origin to  $O''$ , where  $OO'' = b \cdot OJ$ , the equations to the circle become

$$O''P = x'' \cdot OI + y'' \cdot OJ, \quad x''^2 + (y'' + b)^2 = c^2.$$

The radical loci, when  $y''$  is clinant, will have for their equations

$$O''R = x'' \cdot OI + r'' \cdot OJ, \quad r'' + b = 0,$$

that is, the line  $O_1O$ , and

$$O''S = x'' \cdot OI + s'' \cdot OJ, \quad x''^2 - s''^2 = c^2,$$

that is, the rectangular hyperbola  $S''$ ,  $S''_2$ . Hence the roots will be

$$\frac{O_2R + i \cdot O_2S''_1}{OJ} = \frac{O_2R + Rm'_1}{OJ} = \frac{O_2m'_1}{OJ}$$

and

$$\frac{O_2R + i \cdot O_2S''_2}{OJ} = \frac{O_2R + Rm'_2}{OJ} = \frac{O_2m'_2}{OJ};$$

and the factors corresponding to them in the product (5) will be, after replacing  $OJ$  by  $JO$ , as there shown,

$$\frac{O_2O_2 - O_2m'_1}{JO} \cdot \frac{O_2O_2 - O_2m'_2}{JO} = \frac{m'_1O_2}{JO} \cdot \frac{m'_2O_2}{JO}.$$

Hence the final equation in (8) gives

$$\left(\frac{O_1C}{O_2C}\right)^2 \cdot \left(\frac{O_2M'_1}{O_2m'_2} \cdot \frac{O_2M'_2}{O_2m'_1}\right) \cdot \left(\frac{O_2B}{O_1B}\right)^2 = 1.$$

Now the second factor of the product on the left-hand side of this equation, is itself a product of which each factor  $= -1$ , and it may consequently be omitted from the equation, so that

$$\frac{O_1C}{O_2C} = \pm \frac{O_1B}{O_2B}.$$

The under sign cannot be taken, because it would necessitate one of the points  $B$  or  $C$  lying without, and the other within the corresponding side of the triangle. Hence we must have

$$\frac{O_1C}{O_2C} = \frac{O_1B}{O_2B};$$

that is,  $BC$  will be parallel to  $O_1O_2$ , a result which readily follows from other considerations.

12. The great complexity of the process, and the necessity of knowing the algebraical form of the equation to the curve, will probably prevent this employment of clinant roots from having any practical value beyond the completion of the theoretical harmony between the abstract and the concrete, the algebraical formula and the geometrical figure.

## COMMUNICATIONS RECEIVED SINCE THE END OF THE SESSION.

I. "On some Varieties of Tannin." By JOHN STENHOUSE,  
LL.D., F.R.S., F.C.S. Received June 28, 1861.

In two communications "On some Astringent Substances as sources of Pyrogallic Acid," read before the London Chemical Society in the years 1842, 1843, I showed that the usual division of the varieties of tannin into two genera—to wit, those which give black, and those which give green precipitates with salts of iron,—though called in question by Berzelius, is still well-founded : and likewise, that these two genera consist of a great variety of species, which, though closely resembling each other in properties, are still dissimilar in nature ; the only instances in which the same species of tannin had been procured from two different plants, being those of nut-galls and sumach. Professor Strecker's important observation made some seven years ago, that grape-sugar is produced when the tannin of gall-nuts is boiled with dilute sulphuric acid, seemed to render a further examination of the varieties of tannin desirable.

*Sumach.*

As the tannin of Sumach cannot be obtained in such a high state of purity as that of nut-galls by Pelouze's ether process, I was obliged to employ other methods by no means so satisfactory, but which still yield a tolerably pure tannin. A strong decoction of sumach was therefore treated with acetate of lead, which threw down a pale yellow precipitate. This was washed by decantation, and then decomposed by sulphuretted hydrogen and filtered : the filtrate having been boiled with dilute sulphuric acid, on standing for some time, deposited crystals of impure gallic acid. These were collected on a filter, and the mother-liquor, when neutralized with chalk, gave abundant indications of sugar, both by Trommer's test and when subjected to fermentation. A second decoction of sumach, when cold, was treated with sulphuric acid, which threw down a copious precipitate ; this when boiled was also resolved into gallic acid and grape-

sugar. This reaction, therefore, in addition to former experiments, serves to confirm the identity of tannin in oak-galls and sumach.

When sumach is long kept, the tannin it contains appears to be resolved in a great measure into gallic acid and grape-sugar, owing to its undergoing a species of natural fermentation. Accordingly under these circumstances when simply boiled with water, sumach emits an odour resembling that of tea, and yields a large quantity of sugar and gallic acid, but mixed with much impurity. Sumach therefore, though it contains much gallic acid, owing to the impurities present in it, is not a good source of that acid.

I may also remark that the amount of tannin in sumach varies considerably, some specimens being very rich and others very poor. This probably arises from the circumstance that the sumach of commerce is the product of various kinds of *Rhus*, such as *Rhus cotinus*, *Rhus coriaria*, &c.

#### *Chinese Galls.*

These singularly-shaped galls, which are angular and slightly translucent, have been imported into this country in small quantities for the last fourteen or fifteen years. They are said to be found on the branches of a plant which grows in Japan. From the small amount of the colouring matter they contain, Chinese galls are greatly preferred for the manufacture of gallic acid. Oak-galls, on the other hand, are the best source of pyrogallic acid. The decoction of Chinese galls gave a copious white precipitate with acetate of lead. This was decomposed by sulphuretted hydrogen, and filtered; the clear liquid, when boiled with sulphuric acid, was resolved into gallic acid and grape-sugar, as in the two preceding instances. Another portion of the filtered liquid was evaporated to dryness, and destructively distilled. It yielded a very large quantity of pyrogallic acid. It is clear, therefore, that gall-nuts, Chinese galls, and the various kinds of sumach, contain the same species of tannin.

#### *The Tannin in Tea.*

The tannin both of green and black tea is invariably accompanied by a small quantity of gallic acid, which does not arise from the decomposition of the tannin in the tea, as is the case with nut-galls, sumach, or Chinese galls. On treating a strong decoction of tea, when cold, with nearly half its bulk of sulphuric acid, the tannin falls as a

dark brown precipitate. This was collected on a cloth filter, strongly compressed, and washed with a little cold water to free it as much as possible from adhering impurities. The precipitated tannin, when boiled with dilute sulphuric acid, did not yield a trace either of grape-sugar or gallic acid, but was changed into a dark brown substance nearly insoluble in water. It dissolved pretty readily in alcohol, forming a dark brown solution, from which, however, no crystals could be obtained.

*Oak-bark from Quercus pedunculata.*

A decoction of oak-bark gave a dark brown precipitate with acetate of lead. This was decomposed by sulphuretted hydrogen. The filtered solution, when boiled with sulphuric acid, yielded grape-sugar and a reddish-brown precipitate, but no gallic acid. The reddish-brown precipitate dissolved with difficulty in spirit of wine, but gave no crystals on standing. Rochleder has stated that tea and oak-bark contain the same species of tannin. They certainly resemble each other in their general characters; but as the tannin of oak-bark yields sugar when boiled with dilute sulphuric acid, while that of tea does not yield a trace of sugar, it is clear that the two tannins are not identical.

*Valonia, the acorn of the Quercus Ægilops.*

This species of tannin, which has a bright yellow colour, when treated in the way already described, yielded sugar, but no gallic acid.

The tannin of pomegranate rind also gave abundant indications of sugar, but no gallic acid.

The tannin of Myrobalans, the fruit of *Terminalia Chebula*, gave similar results.

*Tannins which give green precipitates with persalts of Iron.*

*Salix triandra* and *Salix undulata* (the willows generally used for basket making).—A quantity of the bark of these two willows, when boiled with water, yielded a dark brown solution. This, when treated with acetate of lead, gave a copious precipitate of a brownish-yellow colour. This lead-salt was decomposed by sulphuretted hydrogen; and the clear filtered liquid, when boiled with sulphuric acid and

then neutralized with chalk, gave abundant indications of grape-sugar. Another portion of the solution from the decomposed lead-precipitate, when digested with nitric acid, yielded only oxalic acid.

When a decoction of willow-bark is boiled with dilute sulphuric acid, the brown-coloured liquid becomes very red, and a flocculent brownish-red precipitate falls which is nearly insoluble in water, but dissolves pretty readily in hot spirit of wine and in alkaline leys. The brownish-red precipitate, when dissolved in spirit of wine and left to spontaneous evaporation, did not crystallize, but formed a dark brownish resin. It consisted chiefly of impure saliratine, resulting most probably from the decomposition of salicine in solution. When it was digested with nitric acid it yielded a good deal of nitropicric acid. Willow-bark, therefore, is a tolerably good source of this acid.

*Alder-bark* (*Alnus glutinosa*).—Alder-bark yields a dark red decoction with water. It was precipitated with acetate of lead, and the lead-salt decomposed with sulphuretted hydrogen. It formed a dark red solution, which on digestion with sulphuric acid yielded no sugar.

*Catechu*.—It was the light-coloured cubical variety of catechu that I employed. The tannin from this astringent substance, when digested with dilute sulphuric acid, yielded no sugar, a result which corresponds with Neubauer's experiments (*Ann. der Chem. und Pharm.* vol. xcv. p. 103).

*Larch-bark* (*Pinus larix*, *Linn.*).—The bark of the larch is employed in Scotland to some extent in tanning, though the leather made with it is inferior in quality. Larch-bark contains a good deal of a peculiar tannin, which yields olive-green precipitates with salts of iron. The aqueous solution of larch-bark is strongly acid to test-paper, and has at first a pale yellow colour, which exposure to the air renders brownish-red. Acetate of lead threw down a copious yellow precipitate: this was decomposed by sulphuretted hydrogen and boiled with dilute sulphuric acid, when the liquid assumed a fine scarlet colour, like infusion of Brazil-wood. The altered tannin precipitated on cooling in beautiful red flocks, as it is but little soluble in cold water. It is very soluble in alcohol, and its solution has a rich scarlet colour, which is characteristic of this species of tannin. Its alcoholic solution, when left to spontaneous evaporation, did not yield crystals, but formed a dark resinous mass. The clear liquid from which the red flocks were precipitated, when neutralized with

chalk, gave no indications of sugar. Sugar, however, together with a good deal of mucilage and resinous matter, with some larixinic acid, is contained in the crude decoction of larch-bark.

*Bark of the common black Mangrove* (*Rhizophora Mangle* of botanists).—This species of mangrove-bark is used in tanning, and is occasionally imported into Great Britain for this purpose. It has a brownish-red colour: the colour of its decoction is the same. The tannin it contains is precipitated from its solution both by acetate of lead and concentrated sulphuric acid. When boiled with dilute sulphuric acid no sugar is produced, and the brownish-red precipitate which falls cannot be made to crystallize.

In conclusion, I would observe that it is somewhat remarkable that so many of the tannins which give bluish-black precipitates with persalts of iron are glucosides; whereas of those which give olive-green precipitates with persalts of iron, so far as I know, only one—to wit, the tannin of the willow—is a glucoside.

- II. "On Larixinic Acid, a crystallizable volatile principle found in the Bark of the Larch Tree (*Pinus Larix*, Linn.)." By JOHN STENHOUSE, LL.D., F.R.S., F.C.S. Received July 10, 1861.

(Abstract.)

This acid is prepared by digesting larch-bark in water at 80° C., evaporating the infusion at the same temperature to the consistence of syrup, and distilling it in vessels of glass, porcelain, or silver; i. e. of a material not liable to be attacked by the acetic acid present in the infusion. The larixinic acid distils over, and partly crystallizes on the inner surface of the receiver, but chiefly remains dissolved in the distilled liquid, which, after being concentrated by cautious evaporation, deposits the impure acid in form of crystals. These are of a brownish-yellow colour; they are to be dissolved and recrystallized, and may be obtained quite pure by sublimation, which takes place at the low temperature of 93° C.

This acid exists as a proximate principle in the larch-bark; most abundantly in that from trees of not more than 20 or 30 years' growth, or from the smaller branches of older trees. When pure, it forms beautifully white crystals, often more than an inch long, of a



silvery lustre, and much resembling benzoic acid in aspect. They sublime at  $93^{\circ}$  C. and melt at  $153^{\circ}$  C.; but in aqueous solution the acid volatilizes at ordinary temperatures. The crystals belong to the oblique system, and usually occur in twin form. The measurement of the angles, as determined at the author's request by Professor W. H. Miller, is given in the Paper.

Larixinic acid, like naphthalin and camphor, emits a sensible smell at ordinary temperatures; its taste is slightly bitter and astringent. It very feebly reddens litmus, and a single drop of potash or ammonia suffices to render a large quantity of it alkaline. It is very soluble in boiling water, but takes 87 or 88 times its weight of water at  $60^{\circ}$  to dissolve it; it dissolves also in cold alcohol, but much more largely in hot alcohol, and sparingly in ether. The crystals are inflammable, and burn with a bright flame, leaving no residue.

Three analyses of the crystals gave the following results:—

Calculated numbers.		Found.		
$C_{20}$	$= 57.14$ .....	57.13	57.06	57.09
$H_{10}$	$= 4.77$ .....	5.04	5.09	5.04
$O_{10}$	$= 38.09$ .....	37.83	37.85	37.87

The carbon, hydrogen, and oxygen are therefore in the proportions  $C_2$ ,  $H_1$ ,  $O_1$ , and the author adopts  $C_{20}$ ,  $H_{10}$ ,  $O_{10}$  as the most probable actual multiples of these numbers.

With ammonia, larixinic acid forms a combination so feeble as to be severed by the mere volatility of the ammonia; in this respect, as well as in forming no hydrate, resembling pyrogallic and oxyphenic acids. With potash it forms long flattish crystals of a reddish-brown colour, which deepens on recrystallization. This combination is decomposed by carbonic acid; it was not obtained of constant composition. Larixinic acid gives no precipitate with lime-water or saccharate of lime; with baryta it forms a bulky, gelatinous precipitate, like hydrated alumina, easily decomposed by carbonic acid, and found from two experiments to contain 34.92 per cent. of baryta.

Solution of larixinic acid gives no precipitate with basic or neutral acetate of lead, perchloride of platinum (even with heat), nor with nitrate or ammonio-nitrate of silver, although, when boiled with the latter salt, it reduces the silver.

This acid contains no nitrogen ; it does not reduce oxide of copper in Trommer's test ; it dissolves in concentrated sulphuric acid without forming any conjugate combination. When boiled with a mixture of hydrochloric acid and chlorate of potash it is decomposed, but without giving rise to chloranile ; it gives no coloration when boiled with solution of hypochlorite of lime. Nitric acid, especially when aided by heat, attacks larixinic acid, leaving oxalic acid as the only fixed product. When heated with bromine it is destroyed, hydrobromic acid vapours are given off, and an uncrystallizable resin remains. Salts of copper give to solutions of larixinic acid an emerald-green colour, but cause no precipitate ; chloride of manganese produces neither coloration nor precipitation. Characteristic effects are produced by salts of iron : perchloride and persulphate give a beautiful purple colour which stands dilution well ; and larixinic acid becomes in this way an excellent reagent for detecting the presence of iron, even in minute quantity. It does not affect neutral proto-nitrate of mercury in the cold, and on the application of heat no mercury is reduced.

This acid appears to be peculiar to the larch tree ; at least the author has not been able to find any trace of it in the spruce fir (*Abies excelsa*), or in the Scotch fir (*Pinus sylvestris*). It evidently belongs to that small group of substances, of which pyrogallic acid and pyrochatechin, the oxyphenic acid of Gerhardt, are the only other members yet known. It is much less easily oxidizable than oxyphenic acid, which again is less easily oxidated than pyrogallic acid. Larixinic acid volatilizes at a much lower temperature than either of these two substances, from which it also differs in being a ready-formed proximate principle, and not an educt.

III. "On the Great Magnetic Disturbance of August 28 to September 7, 1859, as recorded by Photography at the Kew Observatory." By BALFOUR STEWART, Esq., A.M. Communicated by General SABINE, R.A., Treas., V.P.R.S. Received June 28, 1861.

(Abstract.)

During the latter part of August, and the beginning of September, 1859, auroral displays of almost unprecedented magnificence were

observed very widely throughout our globe, accompanied (as is invariably the case) with excessive disturbances of the magnetic needle.

The interest attached to these appearances is, if possible, enhanced by the fact, that at the time of their occurrence a large spot might have been seen on the disc of our luminary, a celestial phenomenon which we have grounds for supposing to be intimately connected with auroral exhibitions and magnetic storms.

The auroral displays just mentioned were very attentively observed throughout Europe, America, and Australia. In many places these were of the most gorgeous character, and other places were visited by this meteor where its appearance was an event of very rare occurrence. Even from as low a latitude as Cuba we have a description of it by the Director of the Havannah Observatory, accompanied with the remark that only four previous displays had been recorded in the traditions of the island. In not a few instances telegraphic communication was interrupted owing to the current produced in the wires; and in some cases this proved so powerful that it was used instead of the ordinary current, the batteries being cut off and the wires connected with the earth. The descriptions of this meteor, given by various observers, have been collected together by Professor Loomis, and published in a series of papers communicated by him to the American Journal of Science and Arts. From all these accounts there appears to have been two great displays, each commencing at nearly the same absolute time throughout the globe; the first on the evening of the 28th of August, and the second on the early morning of the 2nd of September, Greenwich time.

Magnetic disturbances of unusual violence and very wide extent were observed simultaneously with these displays. These were recorded more or less frequently at the different Observatories; but at Kew there is the advantage of a set of self-recording magnetographs, which are in constant operation, and by means of which the state of the elements of the earth's magnetic force may be known at any moment.

The author then refers to curves, which accompany his paper, and shows that the first disturbance commenced about 10½ on the evening of August 28th, affecting all the elements simultaneously. At about 7½ P.M., 29th August, the violence of this disturbance had somewhat abated, and things remained nearly in this state until

September 2nd, 5 A.M., about which time another very abrupt disturbance simultaneously affected all the elements, and continued with great violence until about 4 P.M. of the same day, when it somewhat abated. The elements, nevertheless, remained in a state of considerable disturbance until September 5th, and only attained their normal positions on September 7th. It thus appears that we have two distinct well-marked disturbances, both commencing abruptly and ending gradually, the first of which began on the evening of August 28th, and the second on the early morning of September 2nd.

These two great disturbances correspond, therefore, in point of time, to the two great auroral displays already alluded to.

The average effect of both of these disturbances was to increase the declination, and to diminish the horizontal and vertical components of the earth's magnetic force.

The author then alludes to a disturbance which took place about 15 minutes past 11 o'clock on the forenoon of September 1st, similar in its mode of action to the two great disturbances already mentioned, but not equalling them in extent or in duration. This disturbance affected the magnetograph simultaneously with the breaking out, near a spot on the sun's disc, of a bright star, which was fortunately observed both by Mr. Carrington and Mr. Hodgson, independently.

The study of the curves furnished by the magnetograph during this great storm, in the author's opinion seems to throw light upon the connexion which subsists between magnetic disturbances, earth-currents, and auroral displays. These curves show that at the commencement of the disturbance, the horizontal and vertical components of the earth's force remained depressed below their normal values for at least seven hours. The curves for this portion of time have also a peculiar serrated appearance, as if on the great wave of disturbance whose period was seven hours, there were superimposed smaller waves whose period might be perhaps only a few minutes, or even less.

Now Mr. C. V. Walker, in a paper communicated to the Royal Society (January 31st, 1861), shows that the earth-currents which accompany auroras are of very short period (say a few minutes), and the auroras themselves also appear to be of a very fitful nature. We

cannot, therefore, well imagine how any combination of auroras and earth-currents could cause the period of seven hours, which the magnetic curves exhibit; while at the same time we cannot fail to associate the rapidly reversed earth-currents with those serrated appearances which the curves present.

The author suggests the following explanation of these phenomena. The earth itself may be likened to the soft iron core of a Ruhmkorff's machine, and the lower strata of the atmosphere to an insulating material interposed between the earth and the upper strata of the atmosphere, which, being very rare, become conducting, and form as it were the secondary coil of this arrangement.

Now suppose a primary current, probably in our luminary, to influence the earth, and suppose its general direction to remain the same for at least seven hours. This current would act on the magnetic matter of the earth in the same manner during these seven hours; and would, therefore, account for the magnetic wave of seven hours' duration.

But although this current has been supposed to remain in the same direction for seven hours, yet we may suppose that its intensity, especially if it have an atmospheric origin, is of a fluctuating character. Now any *sudden* increase or diminution in the intensity of this current, heightened by the iron core on which it acts, *i. e.* the earth, will produce secondary currents,

1st. Along the surface of the earth, which is sufficiently conducting for this purpose.

2ndly. Along the upper strata of the atmosphere, which are also sufficiently conducting.

These will be the earth-currents and auroras which, according to this hypothesis, are therefore due to the fluctuating nature of this primary current; while, on the other hand, the magnetic disturbances are due to its absolute intensity.

- IV. "On the Aquiferous and Oviducal System in the Lamellibranchiate Mollusks." By GEORGE ROLLESTON, M.D., F.L.S., Linacre Professor of Anatomy; and C. ROBERTSON, Esq., Demonstrator of Anatomy, Oxford. Communicated by Dr. ACLAND. Received August 20, 1861.

(Abstract.)

After recapitulating the views which have been held by various authors as to the means by which certain Lamellibranchiata are enabled to distend their muscular foot, the authors of this paper proceed to make a retraction of the opinions they put forward as to the oviducal system in these mollusks in a paper read before the Royal Society, February 3, 1859. But, though they have some reason to agree with M. La Caze Duthiers's views, as expressed in a paper read before the Royal Society, December 15, 1859, so far as the oviducal outlet is concerned; they are not prepared to coincide with that writer in denying altogether the existence in these animals of an aquiferous system distinct from their blood-vessels. Upon this point their views remain much the same as those they enunciated in their paper already referred to, and they may be briefly summed up thus.

They hold that, side by side with, and yet distinct from, the blood-vascular system in the Lamellibranchiata, there exists another system of tubes forming an aquiferous tree, the trunk of which serves as an outlet for the generative products, whilst many of its branches spread throughout the foot into regions not occupied by the organs of reproduction. This system is such an one as the perivisceral chamber has been shown to be by Mr. Hancock in the 'Philosophical Transactions' for the year 1858, spreading itself into ramifications, some of which are, whilst others are not, in connexion with the reproductive glands. The authors allow, and indeed show in the way of experiment, that it is possible for the water in which the animal lives, to become intermingled with the blood within its vessels by the route of the organ of Bojanus, and it will be seen from what has even already been said, that they suppose the aquiferous system to be fed with water by transudation of that fluid from the blood-vessels.

They begin by describing three sets of experiments in the way of injections, to show that water can find its way into the pericardial blood-lacunæ through the organ of Bojanus; and from thence into the vessels which carry the systemic blood towards, and only into those which carry it *towards*, the gills.

The results of two other sets of experiments are next adduced in proof of the non-existence in the blood-vascular system, first, of any lacuna save in this pericardial space; and, secondly, of any communication with the exterior by pores, save such as the two sacs of the organ of Bojanus may be held to represent upon a gigantic scale.

What is new, however, in this part of the paper, is not so much the conclusions as the methods the authors have adopted for demonstrating them.

In proceeding to argue for the existence of a system of tubes distinct from the blood-vascular system, the authors begin by contrasting the appearance which the non-generative part of the foot presents when it is injected, as it is easily, from the orifice which serves as generative outlet, with that which it presents when injected from the blood-vessels.

Secondly, they show that it is possible when the blood-vessels are already fully occupied by an injection of one colour to cause a second, or when the artery and the vein have been filled with differently coloured fluids, a third system of vessels to make their appearance throughout the foot-mass by throwing a differently coloured injection into the oviducal outlet.

Thirdly, microscopic examination of animals thus treated excludes the idea that the fluid thus interposed between and amongst blood-vessels has found its way simply into interstitial spaces left between them and the tissues, as it shows that it is contained within a system of tubes as well defined and limited off from the surrounding tissues, as is the fluid which has been thrown into the blood-vessels themselves.

That the water which has been shown to enter the body by the intermediation of the organ of Bojanus finds its exit by the same route, the authors believe to be rendered in the highest degree improbable, by the fact that they have found it impossible to make fluid pass in the direction this hypothesis postulates; that is, from the blood-vessels, into the pericardium.

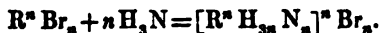
Similar improbability attaches to a view which supposes a fluid of such chemical and such morphological characters as the blood of the Lamellibranchiata, to suffer dilution to such an extent as the observable distention of their foot would necessarily imply; and which argues from phenomena noticed on the sudden removal of the animal from the water, as though they could be regarded as identical with normally occurring physiological processes.

As their injections seem to them to prove the existence of a system of vessels distinct from and yet in most close apposition to the blood-vessels, and permeating the several tissues of the body in company with them, the facts of the case seem to the authors to necessitate the belief that a transference of fluid takes place, as in other organisms, from the latter to the former set of vessels.

The animals experimented upon were Unionidæ of the two species, *Anodonta Cygnea* and *Unio margaritifera*.

V. "Notes of Researches on the Poly-Ammonias."—No. XVI.  
Triatomic Ammonias. By A. W. HOFMANN, LL.D., F.R.S.  
Received July 5, 1861.

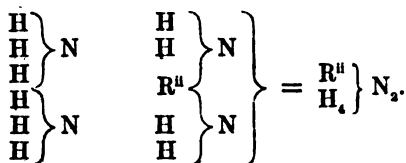
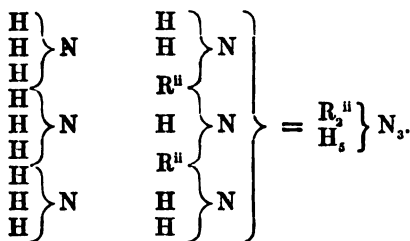
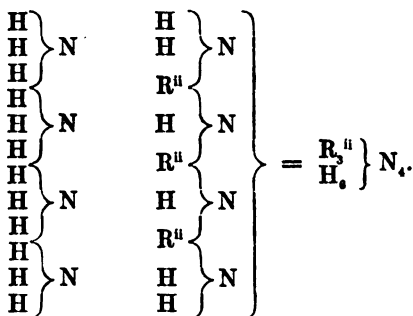
In a short paper which I had the honour of submitting to the Royal Society about a year ago, I endeavoured to delineate the general results to which my experiments on the Polyatomic Bases had led me. I showed that the construction of these bodies may be accomplished in two essentially different ways. In the first place, a number of ammonia-molecules may be joined by the insertion of a polyatomic radical, the atomicity of the radical determining the number of the ammonia-molecules thus united.



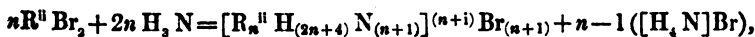
Examples of compounds formed upon this principle have been furnished by my researches on the phosphonias.

Again, the accumulation of the ammonia-molecules may be attempted by increasing the number of polyatomic radicals of given atomicity used as binding material. It is obvious that theoretically any number of ammonia-molecules may be held together by diatomic molecules, provided we appropriately increase the number of the latter.



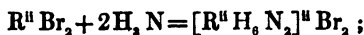
*Diamine.**Triamine.**Tetramine.*

In a general form this second method may be expressed by the equation

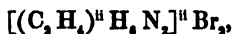


which represents the formation of the first term in each series of a succession of basic groups of increasing atomicity.

The simplest case involved in this equation is the formation of the first term of a series of diammonium-compounds. When  $n=1$ , the above equation assumes the simple form



and if  $\text{R}^{\text{H}} = (\text{C}_2\text{H}_4)^{\text{H}}$ , we obtain in this manner the dibromide of ethylene-diammonium



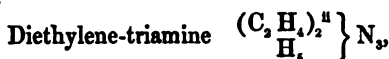
the diamine of which, undergoing further substitution, furnishes the higher members of this class of diatomic bases.

When  $n=2$ , the above equation leads us to the conception of the first term of a series of triammonium-compounds, for

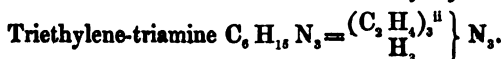
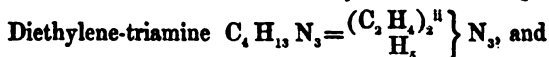


and in attempting the experimental verification of this equation in the ethylene-series, we are justified in expecting, among the products of the action of dibromide of ethylene upon ammonia, the

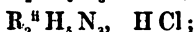
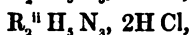
Tribromide of Diethylene-triammonium. .  $[(C_2 H_4)_2^u H_3 N_3]^u Br_3$ ,  
or among the volatile bases liberated from the bromides by the action of an alkali, the



capable, under the continued influence of dibromide of ethylene, of yielding triamines of a higher degree of substitution. These expectations have been fully borne out by experiment. I have satisfied myself that the volatile bases obtained by the action of dibromide of ethylene upon ammonia, which after several rectifications boil between  $200^\circ$  and  $220^\circ$ , consist almost exclusively of the two compounds,



The separation of these two compounds, which, owing to the proximity of their boiling-points, could scarcely be effected by distillation, was attempted by transforming them into saline compounds. But here an unexpected difficulty presented itself in the observation that the triammonias are capable of forming three classes of salts, which may be generally represented as



and that the platinum-compounds of the triatomic bases exhibit a still greater diversity of composition, these bases being not only apt to form the salts corresponding to the chlorides, viz.—



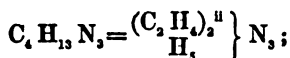
but appearing capable even of forming compounds in which the number of dichloride-of-platinum-molecules diminishes whilst the amount of hydrochloric acid remains constant, such as



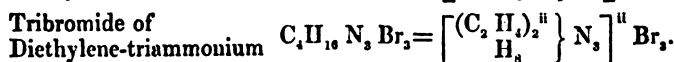
Fortunately most of the salts, and more especially the platinum-compounds, crystallize with remarkable facility, so that the above fact being once established, the circumstances were soon determined under which the more important saline compounds could be with certainty reproduced. The separation of the triamines was chiefly effected in the form of chlorides, bromides, and iodides, or of platinum-salts and gold-salts.

Both diethylene-triamine and triethylene-triamine are powerfully alkaline liquids, soluble in every proportion in water and alcohol, almost insoluble in ether. They boil respectively at  $208^\circ$  and  $216^\circ$ ; but owing to the comparatively small quantities with which I have had to work, these determinations may require some slight corrections. Both bases neutralize the acids completely, giving rise to the formation of well-defined, and in most cases beautifully crystallized salts, which are generally very soluble in water, difficultly soluble in alcohol, insoluble in ether. The aqueous solution of the salts is not precipitated by the solutions of the alkalies, owing to the solubility of the bases; but solid hydrate of potassium separates them in the form of nearly colourless oily liquids, which rapidly attract carbonic acid from the atmosphere.

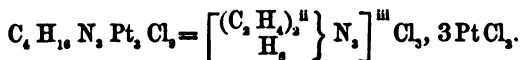
The composition of diethylene-triamine was fixed by the analysis of the free base itself, which was found to be anhydrous,



and of the triatomic chloride, bromide and iodide, which are all remarkable for the facility and beauty with which they crystallize. These salts contain respectively,—

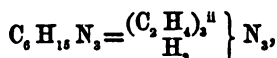


The platinum-salt of diethylene-triamine crystallizes in magnificent golden-yellow needles of the composition



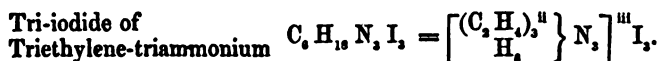
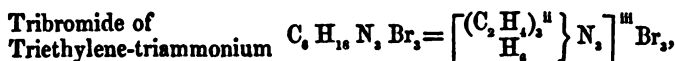
The platinum-salt of this, as well as of several other triammonias which I have examined, cannot be recrystallized without at least partial decomposition. New platinum-compounds are thus produced, in some of which the chloride is united with a smaller number of molecules of dichloride of platinum, whilst others, almost insoluble in water, to judge from the enormous amount of platinum which they contain, appear to be produced by platinic substitution.

Not less definite are the results obtained in the analysis of triethylene-triamine, although, owing to the greater solubility of the compounds of this base and its tendency to form imperfectly saturated salts, the preparation of these substances presents greater difficulties. The analysis of the free base has furnished numbers agreeing with the formula



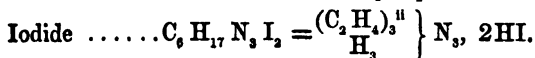
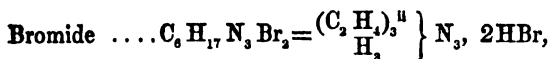
which shows that this, like the diethylenated compound, is anhydrous. It deserves to be noticed that the tendency to form definite hydrates, so prominent in the diamines, is not met with in the case of the triamines.

I have examined numerous salts of triethylene-triamine, both triatomic and diatomic, which confirm the formula of the base. In the presence of a large excess of hydrobromic and hydriodic acids, this substance forms well-crystallized triatomic compounds containing respectively,

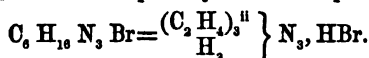


The solutions of these salts are powerfully acid.

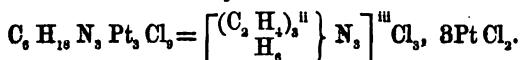
From feebly acid solutions, on the other hand, salts are deposited in which only two equivalents of hydrobromic or hydriodic acids are present. These substances contain



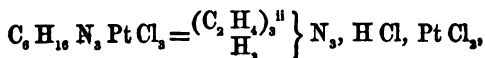
The addition of free base to the solutions of the diacid salts still further reduces the bromine and iodine. I have not, however, succeeded in obtaining the monacid compounds in a state of purity, but the analysis of mixtures leaves but little doubt regarding the existence of this class, and more especially of the compound



In addition to the above salts, the triatomic platinum-salt and gold-salt have been submitted to analysis. The former salt contains

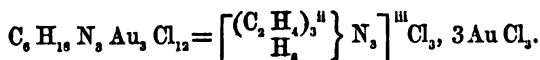


It is one of the finest compounds of the group, crystallizing, as it does, in long golden-coloured needles. They are rather soluble in water, so that the purity of the triethylenated base may be best tested in the form of this platinum-salt, since the platinum-salts of the ethylene-diamines, as well as of diethylene-triamine, with which bases the second triamine is likely to be contaminated, are far less soluble in water. The platinum-salt cannot be recrystallized without suffering decompositions similar to those which alter the salt of the diethylenated triamine. Left in contact with an excess of the corresponding chloride, this platinum-salt undergoes a peculiar metamorphosis. The slender needles, after the lapse of a day or two, are found converted into well-formed prisms of considerable dimensions, containing about 8 per cent. of platinum less than the original compound. The salt appears to be the monatomic platinum-compound,



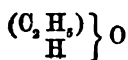
although, owing to the transformations which the salt undergoes when treated with water, I have never been able to obtain it in a state of perfect purity.

The gold-salt of triethylene-triamine crystallizes in yellow plates, soluble in water, alcohol, and ether, which contain

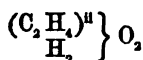


They may be recrystallized from water ; only on protracted ebullition they are decomposed with separation of metallic gold.

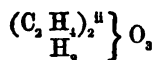
The triatomic ammonias, the history of which I have endeavoured to trace in this paper, are naturally connected with the triatomic alcohol discovered by Wurtz, and described by him under the name of diethylene-alcohol. Diethylene-triamine and triethylene-triamine occupy in the series of diethylene-alcohol the position which is held by ethylamine and diethylamine, by ethylene-diamine and diethylene-diamine in the series of ethylic and ethylenic alcohols respectively,



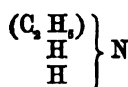
Ethyl-alcohol.



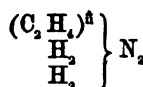
Ethylene-alcohol.



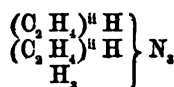
Diethylene-alcohol.



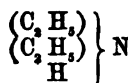
Ethylamine.



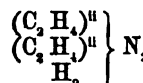
Ethylene-diamine.



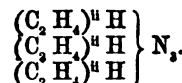
Diethylene-triamine.



Diethylamine.



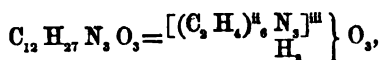
Diethylene-diamine.



Triethylene-triamine.

The above formulæ disclose the perfect symmetry which obtains in the construction of the diatomic and triatomic ammonias ; they also show the number and diversity of the ammonias of increasing atomicity.

If the substitution be carried to the furthest limit, viz. to the complete replacement of the hydrogen in the type ammonium, ethylenic substitution in the diatomic derivatives produces *four* compounds, in the same manner as ethylic substitution in ammonium itself. The group of triatomic ethylene-ammoniums does not comprise less than *five* compounds, the last term of the series being a non-volatile body containing *six* molecules of ethylene and represented by the formula



the existence of which is sure to be established experimentally by the continuation of these researches.

## VI. "Notes of Researches on the Poly-Ammonias."—No. XVII.

Mixed Triammonias containing Monatomic and Diatomic Radicals. By A. W. HOFMANN, LL.D., F.R.S. Received July 29, 1861.

Diethylene-triamine and triethylene-triamine, a short history of which I laid before the Royal Society some weeks ago, are the first terms of a group of a series of triatomic nitrogen-bases, whose construction and composition may be varied almost *ad infinitum*. Similar compounds, there can be no doubt, will be produced by all the homologues of ethylene. With the exception of a few experiments on the action of ammonia on dibromide of methylene, I have not hitherto entered upon this part of the inquiry. Not less numerous is the series of triatomic bases derived from the ethylene-triamines by the further substitution of either monatomic or diatomic radicals for the remaining hydrogen-equivalents. I have satisfied myself that the ethylenated triammonias are powerfully acted upon by the iodides of methyl and ethyl, and likewise by dibromide of ethylene. I have not examined in detail the substances produced in these reactions, but I have had occasion to glance at the ethylated derivatives of the ethylene-triamines when studying the diatomic bases generated by the action of dibromide of ethylene upon the ethylated monamines.

In a former note I have pointed out that the action of dibromide of ethylene on ethylamine gives rise to the formation of

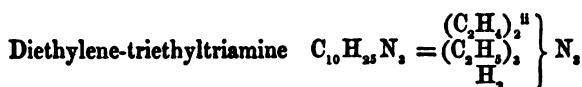


and



These two compounds constitute the chief part of that portion of the volatile bases derived from ethylamine which, after several rectifications, passes over below  $200^\circ$ . The bases boiling above  $200^\circ$  are a mixture which, dissolved in hydrochloric acid, yields with dichloride of platinum a splendidly crystallized platinum-salt. By numerous crystallizations this salt may be separated into one which is difficultly soluble in water, and another which is far more soluble.

The analysis of these platinum-compounds proved them to be the salts of two bases, the formation of which in the above reaction was pointed out by theory, namely, of

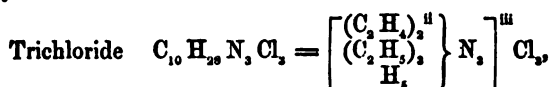


and

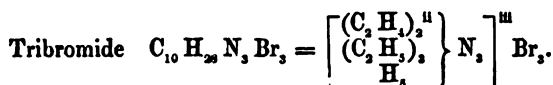


Both these bases are oily liquids, powerfully caustic, extremely soluble in water, which boil between 220° and 250°. I have not prepared them in quantity sufficient for an accurate determination of their boiling-points. Both form neutral crystalline salts which are extremely soluble in water, and also, though less so, in alcohol.

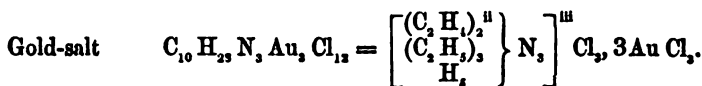
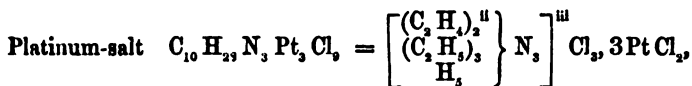
I have established the composition of triethylated diethylene-triamine by the analysis of the chloride and bromide, which contain respectively,



and



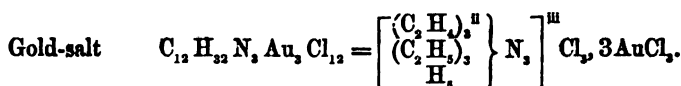
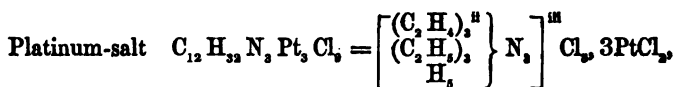
I have also determined the composition of the platinum-salt and of the gold-salt. Both are well-defined beautifully crystalline compounds containing,



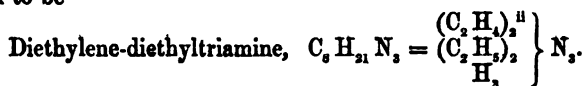
The simple salts of triethylated triethylene-triamine are far more soluble than those of the triamine previously mentioned. I have therefore been satisfied to establish the composition of their base by the analysis of the platinum-salt and of the gold-salt. The platinum-salt is extremely soluble in water, and crystallizes only when the solution is evaporated nearly to dryness. The analysis of these compounds



has led to the following formulæ:—

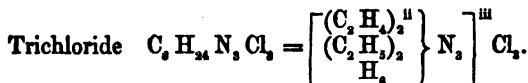


Whilst engaged in the study of the action of dibromide of ethylene upon ethylamine, I occasionally observed among the bases of higher boiling-points an alkaline compound remarkable for the insolubility of its chloride in alcohol. By this behaviour it was easily separated from all the other basic substances accompanying it. This base was found to be

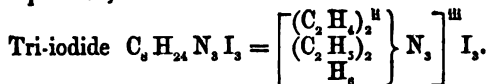


Among the salts of this base are some of the finest compounds which have come under my notice in this inquiry.

The chloride crystallizes in leaves of the lustre of mother-of-pearl, very soluble in water, almost insoluble even in ordinary alcohol, insoluble in ether. It contains,

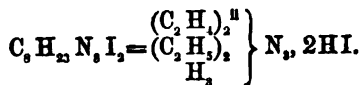


The corresponding iodide is much more soluble, it was found to have the composition,



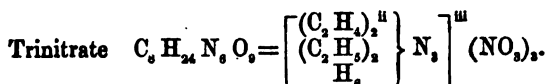
To obtain this salt it is necessary to employ a great excess of hydriodic acid.

The ethylated ethylene-triamines exhibit the same tendency to form diatomic salts which I have pointed out in describing the ethylene-triamines themselves. A solution of diethylene-diethyltriamine just neutralized with hydriodic acid deposits in fact a diatomic salt,

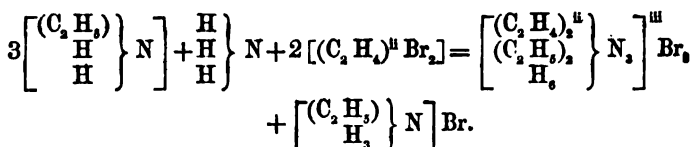


The finest salt of this base is the nitrate. Readily soluble in hot, and moderately soluble in cold water, it is deposited from a saturated

solution on cooling in large rectangular tables presenting the aspect of nitrate of silver. The combustion of this salt has led to the formula



The formation of the diethylated diethylenetriamine is readily intelligible. I have satisfied myself that the origin of this body was always due to the presence of minute quantities of ammonia which had not been entirely removed from the ethylamine employed in the reaction.

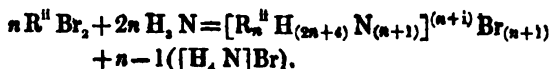


The formation of diethylene-diethyltriamine furnishes an elegant illustration of the simple mechanism involved in the construction of the polyatomic bases.

## VII. "Notes of Researches on the Poly-Ammonias."—No. XVIII.

Tetrammonium-Compounds. By A. W. HOFMANN, LL.D.,  
F.R.S. Received July 29, 1861.

In several previous communications I have submitted to the Royal Society a sketch of the triatomic bases which are generated by the assimilation of three molecules of ammonia linked together by the intervention of diatomic molecules. In the same sketch I endeavoured to trace the general principle upon which the bases of higher atomicity are formed. I pointed out that the accumulation of ammonia-molecules is determined by the number of diatomic radicals which are fixed; that, for the production of an  $(n+i)$ -atomic ammonia, at least  $n$  diatomic radicals are required; and lastly, that the number of molecules of diatomic bromides and the quantity of ammonia involved in these accumulative processes are given in the general equation

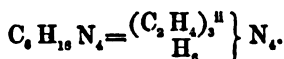


I moreover showed how this equation applies to the first terms of the series of diammonium- as well as triammonium-salts.

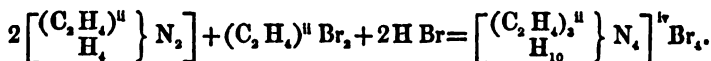
In following up the natural development of these ideas, I had to search for the tetrammonium-compounds. For if  $n=3$ , the above equation reduces itself to



and the simplest tetramine of the ethylene-series—the triethylene-tetramine—is represented by the formula



I have vainly searched for this compound among the difficultly volatile bases resulting from the action of dibromide of ethylene upon ammonia, which distil after the triammonias have passed. But it is obvious that so complex a compound must have a very high boiling-point, and thus be liable to decomposition by distillation. To avoid this decomposition, the product of the action of dibromide of ethylene upon ammonia was treated with an excess of oxide of silver; the liberated bases were then submitted to a long-continued current of steam, by which all the volatile matter was carried over. A considerable proportion of non-volatile bases remained behind, the mixed character of which was easily proved by progressive precipitation by dichloride of platinum. The analysis of these platinum-salts has convinced me that this liquid indeed contains the tetramine in question, mixed, however, with other compounds of very similar properties; so that my endeavours to obtain it in a state of purity have failed. I have, however, succeeded in preparing the pure compound by a somewhat different process, viz. by submitting ethylene-diamine, instead of ammonia, to the action of dibromide of ethylene. The formation of the tetrammonium-compound in this reaction is represented by the equation



The hydrobromic acid which figures in this equation arises from another phase of the reaction, which I have not yet completely studied.

Triethylene-tetramine is a powerfully alkaline liquid which is separated from the bromide by means of oxide of silver. It dries up to a syrup, which shows no tendency to crystallize. Its composition

was fixed by the analysis of the pale-yellow, amorphous, almost insoluble platinum-salt, which was found to contain



Although less intricate than the result of the reaction between ammonia and dibromide of ethylene, the products obtained under similar circumstances from ethylene-diamine present still considerable complexity; and the impossibility of separating the tetramines by distillation, together with their rapidly diminishing faculty of yielding crystalline compounds, render the investigation of these substances rather difficult, more particularly since time, temperature, and even relative proportion of the reacting compounds are apt to vary the nature of the products formed.

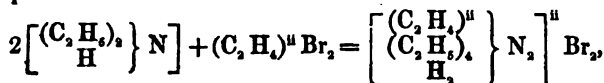
A simplification of these difficulties appeared to present itself by adopting the method which had so greatly facilitated the study of the bases of lower atomicity, viz. by examining the deportment of some of the ethylated monamines under the influence of dibromide of ethylene.

As a chemical theory expands and becomes more and more consolidated, the interest attached to the individual compounds used as scaffolding in raising the structure becomes less and less, diminishing in fact in the inverse ratio of the number of the compounds which the theory suggests. It thus became a matter of comparative indifference, in what series and by what materials the construction of the group of tetrammonium-compounds was attempted. Ethylamine and diethylamine appeared to recommend themselves by their accessibility. Owing to the greater simplicity of the reaction, I will first mention the results elicited in studying the deportment of the diethylated monamine.

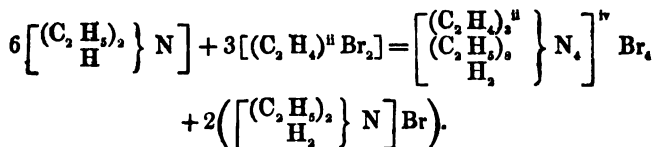
#### *Action of Dibromide of Ethylene upon Diethylamine.*

The reaction is rapidly accomplished, both in the absence and presence of alcohol. On opening the tubes after several hours' digestion at 100°, the liquid is found to be acid; the powerful evolution of bromide of vinyl, which is invariably observed, at once points out the existence, among the products of the reaction, of considerable quantities of bromide of diethylammonium. In addition to this bromide, the reaction of dibromide of ethylene upon diethylamine furnishes

only two other bromides, viz. the dibromide of ethylene-tetrethyldiammonium and, lastly, the tetrabromide of triethylene-octethyltetrammonium. The following equations represent the formation of these compounds:—

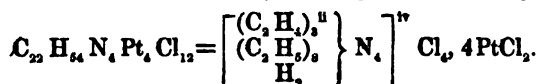


and

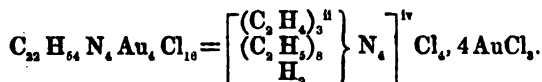


The separation of these three salts presents no difficulties. Liberated by oxide of silver and submitted to a protracted current of steam, the mixture of bases separates on the one hand into diethylamine and ethylene-tetrethyldiamine, which pass over with the steam, and hydrate of triethylene-octethyltetrammonium, which remains behind.

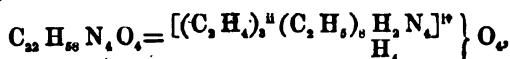
The octethylated tetrammonium-compounds are remarkable for their definite characters: they are still crystalline,—a circumstance which has facilitated their study in no common degree. The composition of the series was fixed by the analysis of the platinum-salt, of the gold-salt, and of the tetr iodide. The platinum-salt is almost insoluble in water. Precipitated from a dilute and gently warmed solution, it is obtained in small crystalline plates containing



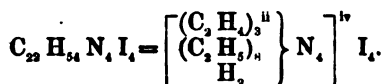
This substance has served in the preparation of the other salts. Treated with sulphuretted hydrogen, the platinum-salt is converted into the difficultly crystallizable chloride, which yields with trichloride of gold a crystalline gold-salt of the composition



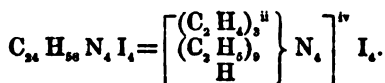
The chloride, submitted to the action of oxide of silver, furnishes the powerfully alkaline free base



which has all the characters of the non-volatile monammonium-, diammonium-, and triammonium-bases which I have previously described. Treated with hydriodic acid, it is converted into the tetriodide, which is very soluble in water, but which may be obtained from alcohol in white exceedingly soluble crystals, of the composition



It deserves to be noticed that the tetrammonium-compound, which I have endeavoured to sketch, does not contain more than three molecules of ethylene—that it is, in fact (if we disregard the accidental circumstance of its octethylated condition), the simplest tetrammonium-compound which could possibly be formed, three molecules of ethylene being, as is evident from a glance at the general equation given at the commencement of this note, the smallest number of diatomic molecules by which a tetrammonium-compound may acquire the necessary stability. I have submitted the free octethylated base to the action of iodide of ethyl. This treatment gives rise to a beautifully crystallized iodide less soluble in alcohol, which contains—

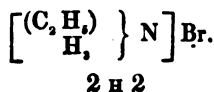


I have not carried the ethylation any further.

#### *Action of Dibromide of Ethylene upon Ethylamine.*

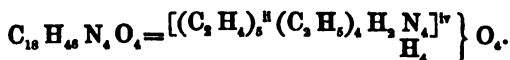
The reaction between these two bodies, as might have been expected from the number of unreplaced hydrogen-equivalents in ethylamine, is far more complex than the process previously examined. The action is rapidly accomplished at 100°; and it deserves to be noticed that very little bromide of vinyl is formed in this operation, only traces of gas being evolved on opening the digestion-tubes. The crystalline mass which remains on evaporating the product of the reaction to dryness, is a mixture of six, and occasionally of seven bromides, viz.

Bromide of ethylammonium

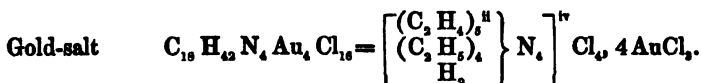
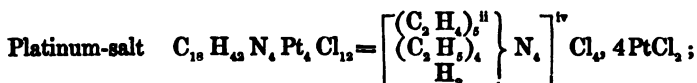




The ammonias corresponding to the first five of these bromides are known by former researches. They are all volatile; there was therefore no difficulty in separating them from the mixture. Liberated by means of oxide of silver and submitted to the action of steam, they were carried over, leaving behind a powerfully alkaline liquid, which in most cases was found to consist exclusively of the hydrate of pentethylene-tetrethyltetrammonium,

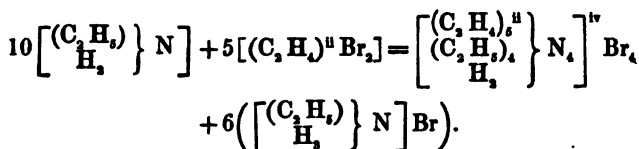


The simple salts of this tetrammonium are extremely soluble, and crystallize with the greatest difficulty; I have therefore established the composition of the series by the analysis of the difficultly soluble platinum-salt and gold-salt. They are obtained in the form of yellow, amorphous or very indistinctly crystalline precipitates, containing,

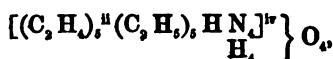


The formation of the pentethylenated tetrammonium is represented

by the following equation :—



The pentethylene-tetrethylated tetrammonium contains still 2 equivalents of replaceable hydrogen. By the action of iodide of ethyl they may, although with difficulty, be removed and replaced by ethyl. I have thus obtained successively the pentethylene-pentethylethylated and, lastly, the pentethylene-hexethylethylated tetrammonium,—

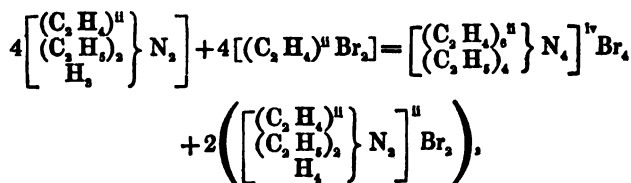


and

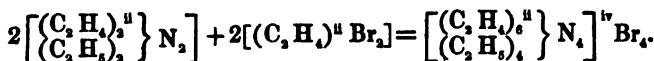


the composition of which was established by the analysis of the platinum- and the gold-salts.

I have mentioned above, that the action of dibromide of ethylene upon ethylamine produces occasionally also the hexethylene-tetrethyl-tetrammonium. This substance may be obtained in a state of purity by the action of dibromide of ethylene upon ethylene-diethyldiamine and diethylene-diethyldiamine,



and



The composition of the hexethylene-tetrethylated tetrammonium was likewise established by the analysis of the platinum-salt and gold-salt.

The analysis of these salts completes my researches on the tetrammonium compounds. In a further paper I propose to examine the ammonias of higher atomicity.



VIII. "On the Lead-Zinc and Bismuth-Zinc Alloys." By A. MATTHIESSEN, F.R.S., and M. VON BOSE. Received August 28, 1861.

The fact that lead and zinc, and bismuth and zinc do not alloy together in every proportion is well known, but there have been, we believe, no determinations made as to the extent in which these metals alloy with each other.

The following experiments were made to ascertain quantitatively what amount of zinc will dissolve in lead and bismuth, and, on the other hand, the amount of bismuth and lead in zinc.

The metals\* were fused in a Hessian crucible over a 4-Bunsen burner, stirred with a tobacco-pipe stem for a quarter of an hour, and then allowed to remain quiet for half an hour in a fused state: during the whole time a jet of gas was directed on the surface of the melted metals. They were after this cast in a porous cell, which had been previously heated to redness in a large crucible filled with sand. It was generally about two hours before the metals became solid: in some cases the crucible was placed in a furnace with a low fire so as to cool much slower, but the separation of the metals did not appear to be more perfect than when cooled in the usual manner. When cold the cell was broken, and the top of the alloy separated from the lower part by a blow of the hammer.

The weight of each casting was about 300 grms., its height about 100 millims., and its diameter about 25 millims.

*Lead-Zinc Alloys.*

*The Zinc-end.*—About 12 grms. were taken from the middle of the end, avoiding the outside. The greater part of the zinc was dissolved in dilute hydrochloric acid, the residue dissolved in nitric acid and precipitated by sulphuretted hydrogen; the washed sulphide oxidized by fuming nitric acid, and the lead weighed as sulphate. Or, instead of precipitating by sulphuretted hydrogen, the mixed nitrates were precipitated by ammonia and carbonate of ammonia, and the lead weighed as oxide.

*The Lead-end.*—8 grms. were dissolved in nitric acid, precipitated by sulphuretted hydrogen, the filtrate evaporated almost to dryness,

\* Purified as described in the Phil. Trans. 1860, p. 177.

and again treated with sulphuretted hydrogen ; the zinc then thrown down by carbonate of soda and weighed as oxide.

*Analyses of the Lead-zinc Alloys.*

Lead 50 per cent., and zinc 50 per cent.		
	Found.	Per-centage.
Taken lead-end... 8·000 grm.	0·162 ZnO	1·62 Zn
Taken zinc-end ... 12·450 „	0·164 PbO	1·22 Pb
Lead 66·6 per cent., and zinc 33·3 per cent.		
Taken lead-end... 8·000 grms.	0·162 ZnO	1·62 Zn
Taken zinc-end... 12·55 „	0·162 PbO	1·20 Pb
Lead 4 per cent., and zinc 96 per cent.		
Top ..... 14·170 grms.	0·247 PbO SO <sup>2</sup>	1·20 Pb
Bottom ..... 19·850 „	0·340 „	1·17 „
Lead 96 per cent., and zinc 4 per cent.		
Top ..... 8·000 grms.	1·63 ZnO	1·63 Zn
Bottom ..... 8·000 „	1·79 „	1·79 „

*Bismuth-Zinc Alloys.*

*The Bismuth-end.*—About 3 grms. were dissolved in nitric acid, evaporated with hydrochloric acid to destroy the nitric acid, precipitated by sulphuretted hydrogen, evaporated almost to dryness, and again treated with sulphuretted hydrogen ; the zinc was lastly precipitated by carbonate of soda and weighed as oxide. The analyses marked (\*) were made in the following manner :—The alloy was dissolved in nitric acid, diluted with water, and the bismuth precipitated by ammonia and carbonate of ammonia. The bismuth was filtered off and washed with a mixture of 10 parts water, 2 of ammonia, and 1 of carbonate of ammonia. The filtrate was mixed with carbonate of soda in excess, and evaporated down on a water-bath in a platinum dish ; when dry, it was moistened with water and again evaporated to dryness in order to drive off the last traces of ammoniacal salts. The dry mass was then washed into a beaker, made slightly acid, boiled, and whilst boiling precipitated by carbonate of soda : the zinc was weighed as oxide.

*The Zinc-end.*—About 12 grms. were taken, the zinc partly dissolved out by dilute hydrochloric acid, the residue dissolved in nitric acid, and precipitated by ammonia and carbonate of ammonia : the bismuth was weighed as oxide.

*Analyses of the Bismuth-zinc Alloys.*

Bismuth 50 per cent., and zinc 50 per cent.			
Taken.		Found.	Per-centage.
Zinc-end .....	15·520 grms.	0·420 BiO <sup>3</sup>	2·42 Bi
Zinc-end .....	13·990 "	0·386 "	2·48 "
Bismuth-end.....	8·000 "	1·385 "	13·85 Zn
Bismuth 50 per cent., and zinc 50 per cent.			
Zinc-end .....	14·2042 grms.	0·3784 BiO <sup>3</sup>	2·39 Bi
*Bismuth-end ...	3·5060 "	0·3795 ZnO	8·65 Zn
Bismuth 80 per cent., and zinc 20 per cent.			
Top .....	2·990 grms.	0·525 ZnO	14·0 Zn
*Top .....	3·0096 "	0·5295 "	14·1 "
Bottom.....	3·694 "	0·597 "	12·93 "
*Bottom.....	2·9758 "	0·4895 "	13·1 "
Bismuth 80 per cent., and zinc 20 per cent.			
*Top .....	2·5542 grms.	0·5208 ZnO	16·3 Zn
*Bottom.....	2·5356 "	0·4206 "	13·3 "
Bismuth 80 per cent., and zinc 20 per cent.			
*Top .....	2·5778 grms.	0·4548 ZnO	14·1 Zn
*Bottom.....	2·5757 "	0·2830 "	8·8 "
Bismuth 5 per cent., and zinc 95 per cent.			
Top .....	13·230 grms.	0·351 BiO <sup>3</sup>	2·38 Bi
Bottom.....	16·050 "	0·430 "	2·40 "

From the foregoing analyses, it appears that lead will, under the conditions detailed above, dissolve only 1·6 per cent. zinc, and zinc 1·2 per cent. lead ; that zinc will only dissolve 2·4 per cent., bismuth 8·6–14·3 per cent.

If we now take equal parts of lead and zinc, fuse them together, stir them well, and cool the alloy rapidly, we may regard such an alloy as a mechanical mixture of solutions of 1·2 per cent. lead in

zinc, and 1·6 per cent. zinc in lead. And the same may be said of the bismuth-zinc alloys when fused together in proportions greater than those in which these metals dissolve in one another.

IX. "On some Gold-Tin Alloys." By A. MATTHIESSEN, F.R.S., and M. VON BOSE. Received August 28, 1861.

It was observed in a former research\* that the gold-tin alloys had a great tendency to crystallize, and it was deduced from results then obtained that some of them were chemical combinations. With a view to ascertain whether these would crystallize out from the fused metals, the following experiments were undertaken.

The metals† were weighed out in the proper proportions, and fused together in a crucible over a 4-Bunsen burner, a jet of gas playing on the surface from above to prevent the oxidation of the tin. When fused, the lamp was removed and the alloy allowed to cool (the jet of gas still playing upon it) until the surface began to solidify, when the liquid alloy was poured off from the crystals. Of course the two metals were always stirred well, and cast several times before the alloy was crystallized and analysed.

When the metals were fused together in the proportion to form Au Sn<sub>2</sub> (62·9 per cent. Au) and Au Sn<sub>3</sub> (53·1 per cent. Au), no crystals could be obtained in either case. When, however, more tin was added, so as to make the alloy Au Sn<sub>4</sub> (45·9 per cent. Au), a separation took place into a non-crystalline mass with a glassy fracture and a very crystalline one: these may be easily separated from each other by fusion, for the former has a much higher fusing-point than the latter. The alloy containing 43·5 per cent. gold behaved in the same manner. The analyses of the different parts of these alloys are given in the following Table. The gold was determined by dissolving the alloy in nitro-hydrochloric acid, and precipitating the gold from the strong hydrochloric acid solution with sulphite of soda:—

\* Phil. Trans. 1860. p. 170.

† These were purified as described in the Phil. Trans. 1860, p. 177.

Composition of alloy.	Analysis of non-crystalline part in per cent. of gold.			Analysis of crystals.		
	1*	2*	3*	1st crop.	2nd crop.	3rd crop.
45.9 Au 54.1 Sn	} 47.7	48.3	49.0	43.3	43.3	42.5
43.5 Au 56.5 Sn		.....	.....	43.3	43.1	41.5

The weight of the alloy employed for the first experiment was about 300 grms., of which the non-crystalline part weighed about 150 grms.: in the second experiment the weight of the alloy was about 250 grms., and the non-crystalline part weighed about 50 grms.

The addition of more tin did not cause any further separation into two masses. The following Table gives the results of the experiments made with the other alloys:—

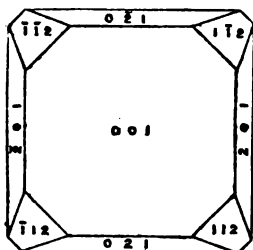
Composition of alloy.	Amount of gold per cent. in first crop of crystals.	Amount of gold per cent. in	Amount of gold per cent. in mother-liquor.
41.8 Au 58.2 Sn	} 43.6	2nd crop. 43.6	40.8
40.5 Au 59.5 Sn		6th crop. 38.7	
37.5 Au 62.5 Sn	} 39.7	4th crop. 37.6	32.9
65.0 Au 35.0 Sn		4th crop. 32.6	30.6
32.5 Au 67.5 Sn	} 36.8	4th crop. 35.2	28.7
30.0 Au 70.0 Sn		6th crop. 31.5	25.3
25.0 Au 75.0 Sn	} 27.4	.....	20.1

\* This was re-fused in order to remove more completely the crystallizable part of the alloy; and as soon as a part had solidified, the rest was poured off. That which first solidified was analysed.

The weight of each of the alloys experimented with was generally about 260 grms., and that of each crystallization on the average 40 grms. In the last alloy, crystals fit for analysis could only be obtained in the first crystallization. The mother-liquor was, however, crystallized four times, and the last mother-liquor analysed.

We are indebted to the kindness of Professor Miller of Cambridge for the following determination of the crystalline form of the foregoing alloys :—

*Description of the Crystals of Gold and Tin.*



Pyramidal :

$$0\ 0\ 1, \ 1\ 0\ 1 = 50^\circ\ 2' \cdot 7.$$

Observed forms :

$0\ 0\ 1, \ 1\ 0\ 2, \ 1\ 0\ 4, \ 3\ 0\ 4, \ 2\ 0\ 1, \ 1\ 1\ 2, \ 1\ 1\ 4,$   
with very obscure traces of the forms

$$5\ 0\ 2, \ 4\ 0\ 1, \ 2\ 0\ 3.$$

The combination most frequently observed was

$$0\ 0\ 1, \ 2\ 0\ 1, \ 1\ 1\ 2.$$

The angles between normals to the faces, computed from a mean of the best observations, are—

$0\ 0\ 1, \ 1\ 0\ 4$	$= 16^\circ\ 37'$
$0\ 0\ 1, \ 1\ 0\ 2$	$= 30\ 50$
$0\ 0\ 1, \ 2\ 0\ 3$	$= 38\ 31$
$0\ 0\ 1, \ 3\ 0\ 4$	$= 41\ 50$
$0\ 0\ 1, \ 2\ 0\ 1$	$= 67\ 16$
$0\ 0\ 1, \ 5\ 0\ 2$	$= 71\ 28$
$0\ 0\ 1, \ 4\ 0\ 1$	$= 78\ 10$
$0\ 0\ 1, \ 1\ 1\ 4$	$= 22\ 53$
$0\ 0\ 1, \ 1\ 1\ 2$	$= 40\ 10$
$2\ 0\ 1, \ 0\ 2\ 1$	$= 81\ 25$
$1\ 1\ 2, \ 1\ \bar{1}\ 2$	$= 54\ 16$
$2\ 0\ 1, \ 1\ 1\ 2$	$= 44\ 17$

The faces of the form 0 0 1 are large and bright ; those of all the other forms are extremely narrow, and are usually uneven.

Cleavage 0 0 1, very perfect.

No other forms of crystals than those just described were observed in any of the alloys experimented with.

The largest and best-defined crystals were obtained from the alloys containing about 41 per cent. of gold. The plates were sometimes, when crystallized from 300 grms., about 30 millims. long and 15 millims. wide, being the height and depth of the alloy in the crucible ; they were generally of a bronze colour, proceeding from a slight oxidation of the tin : their true colour was that of tin. All the alloys emit a grating sound when cut through, as tin does, and are all exceedingly brittle.

From the above experiments it appears, first, that the well-defined crystals are not limited to one definite proportion of the constituents of the alloy, but are common to all gold-tin alloys containing from 43 to 27·4 per cent. gold ; secondly, that crystals and mother-liquor are never of the same composition. These facts coincide with those found by Cooke\* in his research on tin and antimony alloys, who observed that zinc and antimony are capable of uniting and producing definite crystalline forms in other proportions than those of their chemical equivalents.

X. "On the Sensory, Motory, and Vaso-Motory Symptoms resulting from the Refrigeration of the Ulnar Nerve." By AUGUSTUS WALLER, M.D., F.R.S. Received September 8, 1861.

In a brief account of the effects of compression of the human vagus and sympathetic nerves†, I mentioned as one of the symptoms produced, "a tingling and heat of the ear corresponding to the side compressed, often lasting upwards of half an hour after removal of the pressure."

The sensations thus experienced are frequently of a hot, mordicant character, as if arising from the passage of a hot fluid through the vessels, extending progressively and causing a flush over the surface

\* Silliman's American Journal, (2) vol. xx. p. 222.

† Proceedings of the Royal Society, No. 44, page 302.

of the lower part of the ear. The feelings in fact resemble those experienced in ordinary cases of blushing.

In my observations on compression of the vago-sympathetic, I found that the above-mentioned auricular symptoms are more uncertain than those affecting the organs controlled by the vagus. Thus in the same individual where six observations were made successively at about an interval of an hour between each, dyspnœa, irregularity of the heart's action, and uneasiness of the stomach were produced on each occasion, while the ear-symptoms were obtained twice out of the six, and lasted but a few minutes after removal of the pressure.

On the other hand, sometimes the auricular symptoms predominate, whilst the others are scarcely perceptible.

Although the auricular symptoms are certainly due to the influence of some branch of nerve, we find considerable difficulty when we endeavour to ascertain the exact branch that is affected, as besides the vascular branches of the cervical sympathetic which exist over the part compressed, we have also the auricular branches of the cervical plexus close by, which may likewise give rise to the symptoms in question, either by their direct influence or by reflex action.

For many reasons, which I will not enter upon at present, I have come to the conclusion that most, if not all of the symptoms produced by compression of the vagus are the result of reflex action in all the nerves governing the organs affected, viz. heart, lungs, stomach, &c. I have arrived at the same conclusion respecting the mode of production of the auricular symptoms, viz. that they are reflex. As the complex arrangement of the nerves in the neck prevented my obtaining any precise knowledge of the nerve acted upon, I was led to make a careful study of the effects of various agents on those nerves which are more accessible to examination, in order to ascertain how far the spinal nerves distributed to other parts are susceptible of giving rise to the same symptoms as those experienced in the ear.

My present experiments have been made chiefly on the ulnar and internal popliteal nerves, but it is not my intention on this occasion to describe them in detail; I shall accordingly state only the leading results, and for the present shall confine myself to those obtained with the ulnar nerve.



I have generally made use of refrigerating agents, such as ice and water at 0° Cent. The symptoms produced by either of these applications relate to—1st, the sensibility; 2nd, the excitability; 3rd, the temperature of the nerve under examination, or of the parts which it supplies.

*Sensory symptoms.*—The first effect of the application of ice to the ulnar nerve, as is well known, is a state of hyperæsthesia descending apparently to the inner fingers, which gradually increases until it becomes very painful. After a minute or two the hyperæsthesia diminishes, and the pain suddenly disappears.

Sometimes the nerve passes from this condition into a state of complete anæsthesia, when both the nerve and the integuments below are insensible.

In other cases, before these parts are rendered insensible, we have hyperæsthesia and analgesia of the nerve recurring several times before the supervention of the stage of anæsthesia.

*Symptoms of motricity.*—The first change in the excitability of the nerve, or its power of inducing muscular contractions, is that of increased action on either mechanical or electrical irritation, and is so manifest, that an ordinary degree of irritation, which in the normal state will scarcely produce any muscular contraction, will cause strong movements of the wrist and fore-arm.

The second stage of nerve-excitability is that in which there is decreased muscular action terminating in its complete loss.

The passage from the first to the second stage is marked by no distinct symptom, as the one gradually blends into the other. While the nerve is passing through these stages, the muscles subject to the nerve likewise present changes. At the inner part of the fore-arm they slightly contract, and the little and ring-fingers particularly become somewhat flexed and stiff, and their movements imperfect. Finally, these muscular parts become quite paralysed, and the little finger remains permanently flexed.

*Temperature.*—For the purpose of measuring accurately the temperature of the parts supplied by the nerve during the various changes in its condition, very delicate instruments are indispensable. I have used for the present experiments two standard thermometers of M. Geissler of Bonn, in which, although the reservoir is very small, the tenth part of a degree Centigrade is easily read off by the naked eye.

It is almost unnecessary to state that, in measuring the changes of temperature taking place in different parts of an organ such as the hand, two points as nearly homologous as possible in temperature must be chosen. On the hand the intervals between the index and middle finger and that between the annular and little finger are found to present as nearly as possible the same temperature in the normal state.

As thermometric measurements admit of far greater accuracy of expression than those relating to sensibility and muscular action, but at the same time any general or average results require to be founded on a much greater number of observations to be entitled to value, I have thought it preferable, until such have been obtained, to state the numerical thermometric results of a few separate observations.

*Observation 1st.*

Temperature of room .....	19° Cent.
„ of mouth .....	36
„ of palm of hand when shut (before experiment).....	35·7
Temperature between index and median fingers..	35·7
„ „ annular and little fingers of both hands .....	35·7

After the uncovering of the arm, and the application of ice to the right elbow :—

*Right Hand.*

Temperature between last fingers .....	32·2
„ „ index and median.....	32·7

*Left Hand.*

Temperature between last fingers .....	34·3
„ „ index and median.....	34·0

After the lapse of nearly an hour, when the little finger had become nearly paralysed and insensible, the temperature was found to be—

Between last fingers of right hand .....	34·7
„ „ left hand .....	34·7

From this point, as the paralysis gradually became complete, the temperature over the hypothenar eminence, the inner side of the

hand, and the two last fingers became gradually more elevated until it reached 36° Centigrade. At the same time the temperature of the fingers and outside of the hand became lower, at length reaching 28° Cent., where it became stationary. At this time the inner part of the hand presented symptoms of active congestion of blood, being red, and very hot to the touch, and the pulse strong,—symptoms the more evident, by comparison with the outer fingers of the same hand, where the temperature was so much lower, and the pulse weak.

After the removal of the ice from the nerve, sensibility and movement returned into the little finger, &c. in a few minutes without the supervention of any hyperæsthesia.

The restoration of the normal temperature was, however, much more gradual, giving the following results:—

Time after re- moval of ice. Minutes.	Temperature between last fingers. ° Cent.	Temperature between index and median. °
10 .....	33·0 .....	29·0
15 .....	31·0 .....	28·5
17 .....	30·5 .....	28·5
20 .....	29·8 .....	27·5
30 .....	28·8 .....	27·3
40 .....	28·1 .....	27·0
45 .....	27·7 .....	27·0
55 .....	26·8 .....	27·3

Temperature of skin at the internal and external

surface of fore-arm at its lower fourth..... 30

Temperature of mouth ..... 36

„ left hand between last fingers .... 27·7

The preceding observations show that considerable advantages may be derived from *refrigeration* in physiological investigation on man of the functions of the superficial nerves.

The following are the principal results:—

1. The first effect of the application of cold to the ulnar nerve on its sensory function is a state of hyperæsthesia in the course of the nerve.

2. This hyperæsthesia is succeeded by a state of quiescence or freedom from pain, which is followed by anæsthesia.

3. The first effect on the motory functions of the nerve is that of increased excitability.

4. This increase in its turn subsides, until all the parts below the refrigerated portion of the nerve are paralysed, owing to the interruption of its conductivity.

5. In this paralytic condition the muscles remain contracted to a certain extent, and the inner fingers in a state of flexion.

6. The first thermal effect of cold on the nerve is frequently a fall of temperature of  $0^{\circ}5$  Centigrade at the inner fingers below that of the outer fingers.

7. As the nerve becomes paralysed, the temperature of the two inner fingers rises above that of the outer to the extent of  $5^{\circ}$  or  $6^{\circ}$  Cent. This is owing to the paralysis of the vascular nerves and vessels of the parts supplied by the ulnar nerve, and is of the same nature as that in the ear after section of the sympathetic nerve, where, as I have demonstrated, the temperature may be lowered or increased at will by acting on the nerve by galvanism.

8. In my experiments on the sympathetic\*, I have observed an increase of  $10^{\circ}$  Cent. on the side operated on over that of the sound side. In the above observations on man the increase at the inner fingers was  $= 5^{\circ}$  Cent.; and it is probable that in cold weather a much greater difference will be found in the temperature of the two sides, because as the range is between blood-heat, which is the maximum, and the temperature of the part before the experiment, the extent of the range is much greater in cold weather than in the present month of August.

9. While the temperature of the inner part of the hand is rising, that of the outer decreases, on account of the diversion of part of the blood of the radial into the ulnar artery. The same effect is witnessed after section of the sympathetic, where a fall of temperature of the sound ear coincides with elevation of temperature in the opposite ear.

10. After the removal of the refrigerating body, the nerve quickly regains all its original powers, except that the normal equilibrium of temperature of the two sides of the hand is but slowly restored. It is probable that this is to be attributed to the slowness of contraction of the organic muscular fibres of the ulnar artery.

\* *Vide Comptes Rendus de l'Acad. de Sciences*, February 1853.

XI. "Note on the Oxidation and Disoxidation effected by the Peroxide of Hydrogen." By B. C. BRODIE, F.R.S., Professor of Chemistry in the University of Oxford. Received September 9, 1861.

In a former communication\* I laid before the Society a detailed investigation as to the remarkable disoxidation of certain metallic oxides (of which the decomposition of the oxide of silver may be regarded as typical) by the peroxide of hydrogen. I suggested that this decomposition was of a simple and normal chemical character; that the element oxygen was formed according to a molecular law identical with that according to which compound substances are formed; and that the mutual decomposition of the two oxides was determined by the synthesis of the particles of oxygen in opposite polar conditions, according to the equation



After an interval of nearly ten years, this explanation, together with various facts bearing upon the theory of this action, and which were given for the first time in the paper alluded to, have been reproduced as original discoveries by Schönbein, Professor of Chemistry at Basle†.

The reaction does not, however, present itself under this simple form. The amount of oxygen lost by the oxide of silver is a variable quantity, comprised between the extreme limits of the catalytic action in which the reduction is zero, and the normal chemical action as expressed in the above equation. The reason for this variation is to be sought in the disturbing influence of the metal formed during the decomposition.

It is my intention shortly to communicate to the Society a prosecution of the above inquiry, in which I investigate the decomposition by the peroxide of hydrogen of certain oxygenated substances contained in solution, where the perturbing causes which affect the decomposition of the solid oxides and conceal the simplicity of the reaction, do not exist.

The methods by which the following results have been obtained

\* Phil. Trans. ii. 1850, 759.

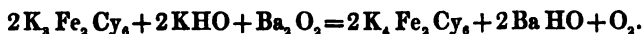
† Phil. Mag. [4] xvi. 178.

will hereafter be given in detail. I confine myself to a brief *résumé* of the conclusions.

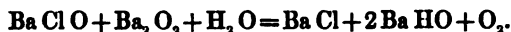
(1) \* When an acid solution of permanganic acid is decomposed by peroxide of hydrogen, the decomposition is in an invariable atomic proportion, according to the equation



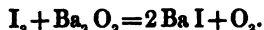
(2) An alkaline solution of ferricyanide of potassium is reduced by the peroxide of barium to ferrocyanide, with the evolution of two atoms of oxygen, thus



(3) An alkaline solution of hypochlorite of barium is reduced by the peroxide of barium, according to the equation



A similar change takes place, as might be anticipated, with an aqueous solution of chlorine in the acid solution of peroxide of barium, but more slowly. I have elsewhere shown that the action of iodine is expressed by the equation



(4) The decomposition of chromic acid is of special interest. In this case there are two distinct reactions, the first of which takes place when chromic acid is in excess, according to the equation



the second, which occurs when a large excess of peroxide of hydrogen is present, and in which the peroxide of hydrogen loses exactly double the amount of oxygen lost by the chromic acid, so that the final result is expressed by the equation



In the interval between these reactions the ratio of the loss of oxygen from the peroxide to the loss from the chromic acid varies between the limits 1 and 2 according to a definite law, being proportional to the ratio of the masses of the substances employed. I am able to show, by direct experiment, that the reaction between

\* This reaction has, I find, been published by Aschoff (see *Repertoire de Chimie Pure*, August 1861, page 296). It was last year made the subject of a communication by me to the British Association, "On the Quantitative Estimation of the Peroxide of Hydrogen."

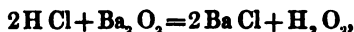
the extreme limits is the sum of two distinct and simple atomic decompositions.

The alkaline peroxides which produce these peculiar effects of reduction, under other conditions act as powerful oxidizing agents; thus an acid solution of ferrocyanide of potassium is oxidized by peroxide of hydrogen to ferricyanide. Hydrated protoxide of manganese is oxidized by peroxide of barium to the condition of peroxide of manganese. A concentrated solution of hydrochloric acid evolves chlorine from peroxide of barium.

This last reaction I have made the subject of a special investigation, with the view of determining the conditions under which the two reactions,



and



respectively take place; and I find that when a solution of hydrochloric acid is boiled with peroxide of barium, if the solution be concentrated, chlorine, if the solution be dilute, oxygen, exclusively is evolved; and that for each intermediate degree of dilution chlorine and oxygen together are evolved in a constant ratio.

It is thus seen that those differences in the behaviour of the different classes of peroxides, from which an imaginary distinction has been drawn between the oxygen respectively contained in them as positive or negative, are not fundamental and characteristic differences. The oxygen in the peroxide of barium is the same as that in the peroxide of manganese, according to the only test of identity which we can apply, for we can produce with it the same effects. Nor are the peculiarities in the reactions of the oxygen of the alkaline peroxides of such a nature as to need any special hypothesis to account for them; for in no case are the combining properties of the particles of matter, like their atomic weights, constant for each chemical substance, but they are variable properties, depending on the physical conditions in which the particles are placed, and the chemical substances with which they are associated.

November 21, 1861.

Sir BENJAMIN COLLINS BRODIE, Bart., President, in the Chair.

In accordance with the Statutes, notice was given from the Chair of the ensuing Anniversary Meeting, and the list of Council and Officers recommended for election was read as follows :—

*President.*—Major-General Edward Sabine, R.A., D.C.L., LL.D.

*Treasurer.*—William Allen Miller, M.D., LL.D.

*Secretaries.*— { William Sharpey, M.D., LL.D.  
 { George Gabriel Stokes, Esq., M.A., D.C.L.

*Foreign Secretary.*—William Hallows Miller, Esq., M.A.

*Other Members of the Council.*—John Couch Adams, Esq., M.A., D.C.L.; Sir William George Armstrong, C.B.; Benjamin Guy Babington, M.D.; Sir Benjamin Collins Brodie, Bart., D.C.L.; George Bowdler Buckton, Esq.; William Benjamin Carpenter, M.D.; Sir Philip de Malpas G. Egerton, Bart.; William Fairbairn, LL.D.; Captain Douglas Galton, R.E.; William Robert Grove, Esq., M.A., Q.C.; William Hopkins, Esq., M.A., LL.D.; John Lubbock, Esq.; James Paget, Esq.; Joseph Prestwich, Esq.; William Spottiswoode, Esq., M.A.; John Tyndall, Esq.

Dr. W. B. Carpenter, Mr. Gassiot, Mr. Lubbock, Dr. W. A. Miller and Dr. John Webster, having been nominated by the President, were elected by ballot Auditors of the Treasurer's Accounts on the part of the Society.

The following communications were read :—

- I. "On some Varieties of Tannin." By JOHN STENHOUSE, LL.D., F.R.S. (See p. 401.)
- II. "On Larixinic Acid, a crystallizable volatile principle found in the Bark of the Larch Tree" (*Pinus Larix*, Linn.). By JOHN STENHOUSE, LL.D., F.R.S. (See p. 405.)



- III. "On the Great Magnetic Disturbance of August 28 to September 7, 1859, as recorded by Photography at the Kew Observatory." By BALFOUR STEWART, Esq., A.M. (See p. 407.)
- IV. "On the Aquiferous and Oviducal System in the Lamellibranchiate Mollusks." By GEORGE ROLLESTON, M.D., and C. ROBERTSON, Esq. (See p. 411.)
- V. "Notes of Researches on the Poly-Ammonias."—No. XVI. Triatomic Ammonias. By A. W. HOFMANN, LL.D., F.R.S. (See p. 413.)
- VI. "Notes of Researches on the Poly-Ammonias."—No. XVII. Mixed Triammonias containing Monatomic and Diatomic Radicals. By A. W. HOFMANN, LL.D., F.R.S. (See p. 420.)
- VII. "Notes of Researches on the Poly-Ammonias."—No. XVIII. Tetrammonium Compounds. By A. W. HOFMANN, LL.D., F.R.S. (See p. 423.)
- VIII. "On the Lead-Zinc and Bismuth-Zinc Alloys." By A. MATTHIESSEN, Esq., F.R.S., and M. VON BOSE. (See p. 430.)
- IX. "On some Gold-Tin Alloys." By A. MATTHIESSEN, Esq., F.R.S., and M. VON BOSE. (See p. 433.)
- X. "On the Sensory, Motory, and Vaso-motory Symptoms resulting from the Refrigeration of the Ulnar Nerve." By AUGUSTUS WALLER, M.D., F.R.S. (See p. 436.)
- XI. "Note on the Oxidation and Disoxidation effected by the Peroxide of Hydrogen." By B. C. BRODIE, Esq., F.R.S., Professor of Chemistry in the University of Oxford. (See p. 442.)
- XII. "On the Contact of Curves." By WILLIAM SPOTTISWOODE, Esq., M.A., F.R.S. Received October 15, 1861. (Abstract.)

In a paper published in the 'Philosophical Transactions' for 1859, p. 371, Mr. Cayley has investigated the theory of the conic of five

pointic contact with any plane curve; but his method does not appear to admit of direct extension to curves of higher orders than conics having contact with any plane curve. The method here suggested would admit of such extension; and it may therefore be said to contain potentially the solution of the general problem of contact. But the labour of completely working out even the case of a cubic of nine-pointic contact would be very great. The case of the conic of five-pointic contact has been worked out, and the result agrees with Mr. Cayley's.

XIII. "On the Calculus of Functions." By WILLIAM SPOTISWOODE, Esq., M.A., F.R.S. Received October 9, 1861.

(Abstract.)

In a paper published in the 'Philosophical Transactions' for 1861, p. 69, Mr. W. H. L. Russell has constructed systems of multiplication and division for functions of certain non-commutative symbols, viz.  $\rho = x$ , and  $\pi = x \frac{d}{dx}$ , and has given the quotient and remainder after both "internal" and "external division" of the symbolical function

$$\rho^n \phi_n(\pi) + \rho^{n-1} \phi_{n-1}(\pi) + \dots \phi_0(\pi)$$

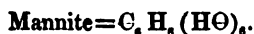
by the factor  $\rho \psi_1(\pi) + \psi_0(\pi)$ . But in the case of

$$\phi_n(\rho) \pi^n + \phi_{n-1}(\rho) \pi^{n-1} + \dots \phi_0(\rho)$$

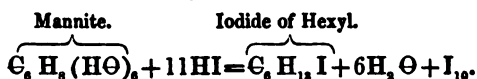
he has given only the quotient and remainder after internal division in the case of  $n=3$ . I have here investigated the general case. The formulæ will be best understood by reference to the memoir itself.

XIV. "On the Action of Hydriodic Acid upon Mannite." By J. A. WANKLYN, Esq., and Dr. ERLÉNMEYER. Communicated by Dr. FRANKLAND. Received October 24, 1861.

Mannite may be regarded as a six-atomic alcohol, or better as *hydride of hexyl*, in which six atoms of hydrogen are replaced by six atoms of peroxide of hydrogen.



This formula is established by the reaction between mannite and hydriodic acid. When the two bodies are heated together iodine is evolved, and a heavy oily liquid distils. This liquid is *iodide of hexyl*. By using a sufficient excess of hydriodic acid—8 grms. of mannite to 100 cubic centimetres of strong acid boiling at 125° C.—and by transmitting carbonic acid through the hot liquid, so as to remove the oily product as rapidly as possible from contact with hot iodine, a quantity of iodide of hexyl, approximating closely to that required by the following equation, may be obtained:—



From 96 grms. of mannite we have obtained 83 grms. of very nearly pure *iodide of hexyl*.

The equation requires 182 grms. mannite to 212 grms. iodide of hexyl.

The identity of our product with iodide of hexyl was shown by its analysis, and by the following reactions:—

Digested with water and oxide of silver, it yields an oily liquid lighter than water, differing completely in smell from amylic alcohol, and having the composition of *hexylic alcohol*. Its analysis gave carbon 70·21 per cent., and hydrogen 13·84.

The calculated numbers for hexylic alcohol are—

Carbon .. ..	70·59,
Hydrogen .. ..	13·73,

which agree completely with our result.

When caustic potash and common alcohol are employed instead of oxide of silver with water, a different result occurs; the iodide splits up into hexylene and hydriodic acid, which latter reacts upon the potash.

Hexylene prepared by this process is an oily liquid, lighter than water, of very penetrating odour, and boiling at about 68° C. Its constitution was proved on the one hand by its analysis and vapour-density, and on the other by its deportment with bromine, upon which it reacts with extreme violence, combining without evolution of HBr, and yielding a heavy liquid, whose composition, ascertained by analysis, is  $\text{C}_6\text{H}_{12}\text{Br}_2$ .

We defer the further description of our compounds, as it is our

intention to make an extended investigation of the bodies belonging to the hexyl group, which are very imperfectly known, as may be inferred from the fact that Faget, the discoverer of hexylic alcohol, has not even published its analysis.

**XV. "The Lignites and Clays of Bovey Tracey, Devonshire."**

By WILLIAM PENGELLY, Esq., F.G.S. Communicated by  
Sir CHARLES LYELL. Received November 16, 1861.

(Abstract.)

The village of Bovey Tracey, in Devonshire, is situated on the left bank of the river Bovey, a small tributary of the Teign, about eleven miles south-westerly from Exeter. A considerable plain stretches away from it, for about nine miles, in a south-easterly direction, and terminates three and a half miles north-west of Torquay. It appears a lake-like expansion of the valleys of the Bovey and Teign, and is surrounded on all sides by lofty hills of granite and other rocks.

Excavations in various parts of this plain, especially in the north-western part of it, known as Bovey Heathfield, have disclosed, beneath an accumulation of gravel mixed with clay and sand, a regular series of strata of lignite, clay and sand, well known to geologists as the "Bovey deposit," whilst the lignite is equally familiar as "Bovey coal."

The most important of the excavations is that known as the "Coal-pit," whence lignite is extracted, which is used, in small quantities, at a neighbouring pottery, and also by the poorer cottagers of the immediate neighbourhood.

The deposit has long attracted the attention of both the scientific and commercial world, and many authors have given descriptions and speculations respecting it.

In 1760 the Rev. Dr. Jeremiah Milles sent a paper on it to the Royal Society. His aim appears to have been to prove the *mineral* origin of the lignite, in refutation of Professor Hollman, of Göttingen, who had described, and assigned a vegetable origin to, a similar substance found near the city of Munden. In 1794 and 1796 Dr. Maton described the deposit, and mentioned the existence of a large turf bog, near the pit, in which whole trees were often discovered,

but "none of them bearing the least resemblance to Bovey coal." In 1797 Mr. Hatchett brought the subject before the Linnean Society, in a paper in which his object seems to have been the refutation of the mineral theory of Dr. Milles. In his 'History of Exeter,' published in 1802, Mr. Brice next gave an account of the deposit, and the state of the lignite workings; he supposed the basin to have formerly been a stagnant lake or morass into which trees were successively transported from the neighbouring slopes. The twelfth letter in Parkinson's 'Organic Remains,' published in 1804, appears to have been written by Mr. Scammell, of Bovey Tracey, and is devoted to the lignite; from it we learn that the coal had been worked upwards of ninety years, and that the trees found in the bog, mentioned by Dr. Maton, were of the fir kind. Mr. Vancouver, in his 'General View of the Agriculture of the County of Devon,' published in 1808, supposes the lignite to have been the product of pine forests which grew where it is found, and that clay and other moveable matter must have been poured over them, in a fluid state, at different periods, from the craggy eminences around. Mr. Austen, in his 'Memoir on the Geology of the South-east of Devonshire,' states that the Bovey beds rest on a gravel equivalent to the lowest tertiary deposits, and is thus the first writer who addresses himself to the chronology of the formation. He makes the overlying gravels post-tertiary, but belonging to the "period prior to the most recent changes of relative level of land and water and of climate." Sir H. De la Beche, in his 'Report of Cornwall, Devon, &c.,' expresses surprise and regret that, excepting the lignite itself, no organic remains have been detected in the deposit, so that we are deprived of any aid by which it may be referred to any particular geological date; and adds that "if the wood be, as has been supposed, analogous to oak and other existing trees, we should suppose the Bovey beds to have been formed towards the latter part of the supracretaceous period." In 1855, Dr. Hooker read a paper before the Geological Society of London, "On some Small Seed-vessels (*Folliculites minutulus*) from the Bovey Tracey Coal," which was the first announcement of the discovery of identifiable fossils in the deposit. Besides the fossil just named, Dr. Hooker described a cone of the Scotch fir, *Pinus sylvestris*, said to have been found in one of the uppermost beds of lignite, and from it he came to the provisional conclusion that the

Bovey beds belong to the Post-Pliocene epoch. In 1856, Dr. Croker, of Bovey Tracey, sent to the same Society a paper in which he mentioned the occurrence of large "flabelliform leaves," together "with tangled masses of vegetable remains in some of the higher beds."

In 1860 Sir Charles Lyell and Dr. Falconer visited Bovey, and returned with the impression that the formation belonged to the miocene age. The latter introduced the subject to Miss Burdett Coutts as one which it was eminently desirable to have fully and carefully investigated. Miss Coutts having soon after visited the district with the author, requested him to undertake an investigation of the deposit, which he accordingly did; and at once engaged Mr. Keeping, the well-known and experienced fossil collector of the Isle of Wight.

Sections of the deposit at the coal-pit show a series of beds naturally dividing themselves into three parts, namely,—

1st, or uppermost, a bed of sandy clay, containing large angular and subangular stones, chiefly of Dartmoor derivation, unconformably covering the lower beds. No stones occur below this.

2nd. A series of twenty-six beds of lignite, clay and sand, the base of which is a bed of ferruginous quartzose sand, in some places 27 feet thick, in others less than one foot, but which everywhere occurs as a well-marked feature in the pit-sections. Excepting this bed, sand is almost entirely confined to the uppermost part of the division.

3rd. A set of forty-five beds of regularly alternating lignite and clay.

The stones by which it is characterized, and its unconformability, show that the uppermost division could not have been formed under the same conditions, nor probably in the same geological period as the two lower series. This view has been confirmed by the identification of certain fossil leaves found in the clays of the uppermost series.

The two lower series are strictly conformable, and dip  $12\frac{1}{2}^{\circ}$  towards S.  $35^{\circ}$  W. (mag.). Five beds,—one of clay and four of lignite,—in the second series, and nine,—one of clay and eight of lignite—in the lower, a total of fourteen, have yielded fossils, all of them remains of plants only. A few only of these beds require particular mention. The seventh bed is, in many places, a mat of the débris of a coniferous tree, the *Sequoia Couttsiae*, and fronds of

ferns, chiefly *Pecopteris lignitum*. The seventeenth bed contains a large number of dicotyledonous leaves. The twenty-fifth is that in which the so-called "flabelliform leaves" of Dr. Croker occur; they have been decided to be large rhizomes of ferns. The twenty-sixth, a bed of clay, is richer than any other in the number and variety of its fossils, which consist of dicotyledonous leaves, seeds of various kinds, and débris of Sequoia stems, leaves, fruits, and seeds. Remains of the last occur, in well-marked specimens, in the fortieth and sixty-third beds; so that it ranges throughout the formation, both divisions of which, therefore, belong to one organic period. The forty-sixth bed abounds in the seeds described by Dr. Hooker as *Folliculites minutulus*, but which Professor Heer has recognized as *Carpolithes Kaltennordheimensis*.

The lignite has frequently a "charred" appearance, and it is difficult to believe that it has not undergone true combustion in the beds, the ignition being spontaneous.

Coal has been found upwards of 170 feet below the surface of the plain, and there are reasons for believing that the deposit is fully 300 feet in depth.

On the advice of Dr. Falconer, the fossils have been submitted to Professor Heer of Zürich, who has found amongst them 49 species of plants, eight of which were found by himself during a personal investigation of the deposit. Many of these are entirely new to science, whilst the others are well known as continental representatives of the lower miocene age. In addition to these, four species have been identified in the uppermost, or gravel division, as belonging to the diluvial period, "that is, a period when the climate of Devonshire was colder than at present."

The deposit is eminently freshwater, and must have been formed in a lake, the bottom of which is, at present, at least 30 fathoms below the level of ordinary spring tide high water. It appears probable that the waters of this miocene lake were separated from the ocean by a barrier which crossed the present tidal estuary of the Teign, and over this the surplus waters passed to the sea; or that they formed a lower outlet in the valley between Newton and Torquay; the first seems, from the physical characters of the two valleys, to have been the most probable course. During the investigation a search was made for fossils at the clay-works of Aller,

Kingsteignton and Decoy, all near Newton Abbot; nothing was found except at the last place, where a stem of *Sequoia Couttsiae* was met with, thus showing that the deposit is identical with that at Bovey Heathfield.

XVI. "The Fossil Flora of Bovey Tracey." By Dr. OSWALD HEEB, Professor of Botany and Director of the Botanical Gardens in Zürich. Communicated by Sir CHARLES LYELL.

(Abstract.)

The surface-covering of the Bovey plateau consists of a light-coloured quartzose sand, which contains here and there considerable beds of white clay. By the plants contained in it this formation is assigned to the Diluvium.

Immediately under it come the beds of clay and lignite, which belong to one formation, far older than that of the overlying white clay; the plants found in them determine them as belonging unquestionably to the miocene period. Hence the formations must be treated of separately.

#### A. *The Miocene Formation of Bovey.*

Of the forty-nine species of plants hitherto discovered in the lignite beds of Bovey, twenty occur on the Continent in the miocene formation. Those beds are therefore undoubtedly miocene. When tabulated, it is seen that fourteen of the twenty species occur in the Tongrien étage, thirteen in the Mayencien, five in the Helvétien, and eight in the Ceningien; hence the Bovey lignites must be ranged in the under miocene, and in the Aquitanien étage of it.

Moreover, the *new* species at Bovey are closely allied to well-known continental forms on this horizon.

It is remarkable that Bovey has no species in common with Iceland, although the tertiary flora of the latter belongs to the same period, and two of its species have been found in the miocene deposit of Ardtun Head in Mull. The Bovey flora has a much more southern character, manifesting, indeed, a sub-tropical climate.

It has certainly some points of connexion with the eocene of the Isle of Wight, but on the whole possesses an essentially different character. The fact that but one species is common to it and Alum Bay, whilst it has so many in common with the more remote miocene



formations of the Continent, satisfies us that it belongs to a different horizon.

Among the twenty-six new species found at Bovey, several interesting forms are found. The first place belongs to *Sequoia Couttsiae*, a conifer which we can illustrate by branches of every age, by cones and seeds. It supplies a highly important link between *Sequoia Langsdorffii* and *S. Sternbergi*, the widely distributed representatives of *S. sempervirens* and *S. gigantea* (*Wellingtonia*), whose occurrence in the present creation is confined to California.

Of great interest also are two species of *Vitis*, of which the grape-stones lie in the clays of Bovey. Three remarkable species of Fig, the seeds of three new species of *Nyssa* and two of *Annona*, one new water-lily (*Nymphaea*), and many highly ornate Carpolithes impart to our knowledge of tertiary plants a most essential extension.

It is highly probable that, at the period of the lower miocene, the Bovey basin was occupied by an inland lake. The entire absence of fresh-water shells, and, indeed, of aquatic animals generally, is certainly very extraordinary, and so is the absence of fruits of the *Chara*, so abundant elsewhere in miocene freshwater deposits; the *Nymphaea* seeds, however, furnish a secure indication of fresh water. We must not omit to notice that the parts of the basin hitherto explored were towards the middle of the lake, and, in the case of the under beds at least, at a considerable depth, which explains the absence of bog-plants as well as of mammalian relics.

The lignite beds consist almost entirely of tree-stems (probably belonging in great measure to *Sequoia Couttsiae*) which have apparently been floated hither, not only from the circuit of the immediate hills, but doubtless also from greater distances.

The twenty-sixth bed in the series, immediately above the "thick bed of sand," is a soft clay with numerous leaves of plants, and ripe cones and seeds of *Sequoia Couttsiae*; the bed was probably formed in autumn, and the plants it contains are due to the driftings of that season. Higher up follows the bed (twenty-five) with fern rhizomes, and occasionally pinnules of *Pecopteris lignitum*. The latter appears in great abundance with branches of *Sequoia* still higher.

As this under miocene formation is immediately succeeded by that of the gravel and white clay, we have here a great hiatus: either the middle and upper miocene, as well as the pliocene periods,

must have passed without the formation of deposits in this place, or they must have been removed again in the diluvial period.

### B. *The White Clay.*

While the lignites and their alternating clays present us with a sub-tropical vegetation, the plants of the White Clay exhibit a totally different character, and must have had their origin in an altogether distinct period.

Four species—three of *Salix* and one of *Betula*—have been found in this overlying mass, no one of which appears to differ from species now living. The presence of the *Betula* (*B. nana*) is conclusive for a diluvial climate, that is, a colder climate than Devonshire has at the present day; for this dwarf birch is an Arctic plant, which has no British habitat south of Scotland, and which occurs in Mid Europe only on mountains and Sub-Alpine peat mosses. The evidence of the willow-leaves is to the same effect, indicating that at this period Bovey Heathfield was a cold peat-moor.

November 30, 1861.

### ANNIVERSARY MEETING.

Sir BENJAMIN COLLINS BRODIE, Bart., President, in the Chair.

Dr. Carpenter reported, on the part of the Auditors of the Treasurer's Accounts, that the total receipts during the past year, including a balance of £512 13s. 3d. carried from the preceding year, amount to £4414 0s. 8d.; and that the total expenditure during the same period amounts to £3729 3s. 7½d., leaving a balance in the hands of the Treasurer of £684 17s. 0½d.

The thanks of the Society were voted to the Treasurer and Auditors.

The Secretary read the following lists:—

Fellows deceased since the last Anniversary.

### *Honorary.*

H.M. Frederick William IV., King of Prussia.

*On the Home List.*

George, Earl of Aberdeen.	The Right Hon. Sir James R.
David Baillie, Esq.	G. Graham, Bart.
Dr. William Baly.	Henry Gray, Esq.
George Bishop, Esq.	Robert William Hay, Esq.
Rev. Edward George Burrows,	George Crauford Heath, Esq.
D.D.	Eaton Hodgkinson, Esq.
Sir William Cubitt.	Sir James Cosmo Melvill.
Rev. James Cumming.	Sir Francis Palgrave.
Rev. Joshua Frederick Denham.	Lieut.-General Sir Charles Wil-
The Right Hon. Charles Tenny-	liam Pasley.
son D'Eyncourt.	Benjamin Phillips, Esq.
Lieut.-General Sir Howard Dou-	John Thomas Quekett, Esq.
glas, Bart.	Robert Rigg, Esq.
William Robert Keith, Lord	Rear-Admiral the Hon. J. Fre-
Douglas.	derick Fitzgerald de Ros.
Thomas Flower Ellis, Esq.	Edward John Rudge, Esq.
William Henry Fitton, M.D.	Alexander Robert Sutherland,
Sir John Forbes, M.D.	M.D.
Hugh, Earl Fortescue.	Rev. Samuel Wix.

*On the Foreign List.*

Friedrich Tiedemann.

Fellows elected since the last Anniversary.

*On the Home List.*

Charles Spence Bate, Esq.	William Newmarch, Esq.
Heinrich Debus, Esq.	Edmund Alexander Parkes, M.D.
Campbell De Morgan, Esq.	William Pole, Esq.
The Right Hon. George Gran-	Philip Lutley Sclater, M.A.
ville Francis Egerton, Earl of	Charles F. A. Shadwell, Capt.
Ellesmere.	R.N.
Thomas A. Hirst, Esq.	Henry J. Stephen Smith, M.A.
A. Matthiessen, Esq.	William Stokes, M.D.
J. Clerk Maxwell, M.A.	George Johnstone Stoney, M.A.
Ferdinand Müller, M.D.	

The President then addressed the Society as follows :—

GENTLEMEN,

THE peculiar circumstances under which the Council thought fit to propose me for re-election as President at the last Anniversary are, I apprehend, sufficiently known to make it unnecessary for me to trouble you with any observations on the subject. There was then reason to believe that in a short time I should be in that state which would enable me to resume all the duties of my office. Unfortunately those expectations were not realized ; and you will, I am sure, easily conceive how deep was the disappointment to myself, that I should have been prevented, during the whole of the last session, from being present at our Meetings. The President of your Society, however, has other duties besides that of presiding in this Chair, and to these I hope that I have not been altogether inattentive. Various subjects connected with the affairs of the Royal Society have from time to time demanded the attention of myself and the other Officers ; and the discussions to which these have led have, I have reason to believe, not been unproductive of a good result.

Referring to the events of the last year, I feel that I may with perfect confidence congratulate you on the position which we occupy. We have not, indeed, had to record any of those grand and startling discoveries by which some former epochs of our history have been distinguished. There never, however, was a period at which so many individuals, well qualified for the task, have been engaged in scientific investigations ; and our published volumes bear ample testimony to the fact, that it is not in one only, but in every department of physical knowledge that a steady progress is being made. The Fellowship of our Society has never been a greater object of ambition than it is at present, nor do I believe that it has ever been held in higher estimation by the public. Our weekly Meetings as I am informed, have been often productive of great interest, and sufficiently well attended : but on this last point I take this opportunity of making a few remarks, in order that I may correct what appears to me to be a great misunderstanding under which some have laboured on the subject.

Probably not more than one third of our Fellows are permanently

resident in London, and of these a large proportion are engaged in occupations which leave only a limited portion of time at their own disposal. The subjects which are brought under consideration, though frequently of paramount importance, are not of that general interest which would attract ordinary persons; nor, indeed, if we take any one of them, can it be regarded as equally attractive to every individual belonging to our own body. In the progress of knowledge, science has become divided and subdivided into many departments, and the principle of the division of labour has necessarily come into operation in these as well as in all other pursuits. A communication, which would be highly attractive to a chemist, would not be equally so to a comparative anatomist or a physiologist; nor would the results of the inquiries on the subject of terrestrial magnetism, which have of late been carried on in almost every part of the world, important as they may eventually prove to be, be well comprehended except by the few who have bestowed their special attention on them. It seems to me, therefore, not reasonable to suppose that the Meetings of the Royal Society should be so numerously attended as more popular assemblies.

There is another matter as to which I conceive that some misapprehension has at times prevailed, namely, the relation in which our Society stands, or ought to stand, to the Government of the country. In many of the continental states there is a society of individuals, under the name of an "Academy," engaged in the pursuit of science, limited in number, and each of them receiving an annual stipend from the public treasury. Such a system may be regarded as offering a premium to those who engage in scientific investigations; and the great results which have been obtained sufficiently demonstrate that where it has been established it has had a very beneficial operation. It would be, however, entirely inapplicable to an institution such as ours, consisting as it does of between 600 and 700 Members. Then the foreign Academies to which I have referred are entirely subjected to the Government, without whose approbation the election of a new member is incomplete. Now, it is plain that a system so inconsistent with the sentiments and habits of the inhabitants of these islands would find little favour here; and I apprehend that there is no individual among those whom I have now the honour of addressing, whatever his opinions on the abstract prin-

ciple may be, who would not think it a most rash proceeding to apply it to the reconstruction of a Society such as ours, which has been steadily advancing in its career, and which is now, after the lapse of 200 years, more active and vigorous and useful than at any former period of its existence.

The best inducement to the cultivation of science is the love of knowledge, combined with that desire of honourable distinction—“*that last infirmity of noble minds*”—of which we must not complain, as it has led to such grand results; and experience shows that these are all-sufficient for the purpose. If any worldly advantage ever accrue to those who are thus engaged, that may be regarded as almost an accidental circumstance, which no one could have anticipated in the beginning.

Such being the state of things among us, I feel bound at the same time to say, that I cannot join with those who complain that the interest of science has been neglected by the Government. The Fellows of the Royal Society have never wished to forfeit their independence, by claiming, in their capacity as Fellows, any personal benefit for themselves. The connexion of the Royal Society with the Government has been simply that of a mutual interchange of good offices. On the one hand, the Society has always been ready to assist those who are intrusted with the management of public affairs with its opinion and advice; and the occasions on which such assistance has been required have been sufficiently numerous. On the other hand, it has rarely, if ever, happened that any representation made by the Royal Society in the interests of science has not received the attention which it required. A sum voted annually by Parliament for the purpose of assisting those who are engaged in scientific investigations has been placed at the disposal of the Royal Society, to be distributed at the discretion of the Council. The extensive suite of rooms which we now occupy are the property of the State, and a similar accommodation has been afforded to four other of the chartered scientific societies; while another more popular institution has an annual grant from the public treasury, in order that it may be the better enabled to carry out the objects for which it was established.

There is nothing by which the pursuit of science in the present day is more signally distinguished than the greater accuracy and

precision with which those investigations to which mathematical reasoning is either not at all or only to a limited extent applicable are conducted as compared with what was the case when men's minds were first directed to these subjects ; nor is there any prospect of our ever returning to those hypothetical systems which prevailed among scientific inquirers formerly. If there be at present any danger to the cause of science, it is of a totally different kind. The time has arrived when the discoveries of science are becoming extensively applied to commerce and manufactures, and the arts of common life. The greater part of society contemplate the achievements of science under this point of view, and estimate its value only as it affects the material interests of the country or of themselves. The prevailing study of political economy—and I say this without denying the great advantages which the community has derived from this comparatively new science—by directing men's minds so much to the increase of national wealth, as the object most deserving of our attention, has the effect of promoting the extension of this utilitarian principle more widely among us. The danger to which I allude is, that the cultivators of science might themselves be led to participate too largely in these utilitarian views. If it should be so, science must undoubtedly descend from the high station which it at present occupies. Nor can this happen without great injury to the cause of knowledge itself. The mere utilitarian philosopher, having his views limited to some immediate practical result, might, like the alchemists of old, elicit some new facts, but would discover no new principle, and after a long life would know no more of the laws of nature than he knew in the beginning. Eventually, even as to their gross material interests, society would be a loser. The sailor would never have had placed at his disposal the means of ascertaining the longitude, if philosophers, without reference to this object, had not studied mathematics and the laws of planetary motions ; nor would London and Paris have ever been placed, as they now are, in instantaneous communication with each other, if those who began with the simple fact of the muscles of a frog's leg being made to contract by the contact of certain metals had not pursued these inquiries until they reached the laws of Voltaic electricity, never dreaming of the great invention which was ultimately to arise out of these researches in the shape of the electric telegraph.

The time has arrived for my resigning into your hands the office to which you were pleased to elect me three years ago. This has not only been the greatest honour which has ever been bestowed on me, but it has also been one of the most gratifying circumstances of my life, to have received such a testimony of the good opinion of individuals so distinguished for their genius and knowledge as the Fellows of the Royal Society of London. I have sincerely to thank not only the other Officers, but the Fellows generally for numerous marks of attention and kindness, of which I am all the more sensible in consequence of the peculiar circumstances under which I have been placed.

The Copley Medal has been awarded to Professor Louis Agassiz of Boston, in the United States, for the eminent services which he has rendered to various branches of physical science by the incessant labours of more than thirty years of scientific activity.

Commencing his career as a zoologist, Professor Agassiz early turned his attention to Ichthyology, and his '*Histoire Naturelle des Poissons d'eau douce*' not only was in itself a very valuable work, but doubtless led the way to the still more important services which Professor Agassiz was destined to render to the same department of natural history—not the least of which was the great step in ichthyological classification, made by the establishment of the order of Ganoids, a group which has now taken a permanent place in the *Systema Naturæ*.

The '*Monographie d'Echinodermes*,' published between the years 1838 and 1842, and the '*Nomenclator Zoologicus*,' which appeared in the latter year contemporaneously with other investigations of quite a different character, need only be mentioned to bear witness to the remarkable combination of originality, industry, and versatility which characterizes their author. To these excellent qualities may be added tenacity of purpose; for after a long interruption, Professor Agassiz, on his removal to the United States, resumed on a great scale those investigations of the lower forms of animal life which had occupied his younger days. The results of these inquiries, and those of his fellow-labourers Clark and Weinland, are embodied in the magnificently illustrated monographs entitled "*Contributions to the Natural History of the United States*," works



which do equal credit to the naturalists who planned them, and to the State and people whose intelligent munificence renders their publication possible.

Cuvier's great work, the '*Ossements Fossiles*,' embraces, as is well known, an account of the fossil remains of all the higher classes of vertebrated animals; but the founder of palæontology left the difficulties of fossil ichthyology to be grappled with by others, and discerning the especial aptitude of Agassiz for the undertaking, indicated him as his continuator in this department. Nor can it be denied that the author of the '*Recherches sur les Poissons fossiles*,' and the '*Monographie des Poissons fossiles du vieux grès rouge*,' has amply justified the sagacious anticipation of Cuvier. Travelling over the Continent and these islands from one collection to another—never possessing specimens of his own, but obliged to trust to notes and to the sketches of the excellent artist who accompanied him—dealing with remains which were almost always fragmentary and presented far less definite characters to the anatomist than the bones of higher animals,—Professor Agassiz, nevertheless, succeeded, in the course of eleven years, in producing works which form a worthy continuation of the '*Ossements Fossiles*;' and this not merely on account of the excellent descriptions and figures of fossil fish, in vast number, which first appeared in their pages, but because associated with the history of extinct forms are all the complementary investigations into the osteology, dentition, and scale-structure of their recent allies required for their elucidation.

The award of the Copley Medal for these investigations alone would be regarded but as an act of justice by the students of Palæontology, but it must not be forgotten that Professor Agassiz has made many other contributions of no slight value to this branch of science.

It might be supposed that labours of such magnitude and difficulty as those which have just been mentioned, would suffice to give full occupation to one mind, whatever its activity; but while Professor Agassiz was thus becoming familiar to zoologists and palæontologists as one of the most active members of their confraternity, geologists and physical geographers knew him as a vigorous worker and bold theorist in their departments. In fact, what is now known as the '*Glacial Theory*,' although not altogether originated by Professor Agassiz, was greatly extended by him, and assuredly owes the position

it has acquired in science in very great measure to his efforts—his work, ‘*Theorie der erratischen Blöcke der Alpen*’ (1838), his other writings, and his genial eloquence at scientific meetings, having done more than anything else to attract the attention of geologists to the efficiency of ice as a modifier of the earth’s surface, and as a means of transport. Thus led to the consideration of glacial phenomena in general, Professor Agassiz, accompanied by M. Desor and others, next devoted many months of several successive years to the systematic study of the glaciers of his native country—especially to that of the Aar, where under the mighty boulder nicknamed by those whom it sheltered the “*Hôtel des Neufchâtelois*,” inquirers of all nations found not only a welcome, but the nobler hospitality of a free access to all that was being thought and planned and done by the little party of savans of whom Agassiz was the head. The results of the investigations thus laboriously carried on, were embodied in the ‘*Études sur les Glaciers*,’ published in 1840, and the ‘*Nouvelles Études*,’ which appeared in 1847; and apart from all minor contributions, Professor Agassiz will be admitted by all who duly acquaint themselves with the history and present state of the question, to have made two important additions to our knowledge of glaciers. For, in the first place, the general account of the glaciers of the Aar given by Professor Agassiz in the ‘*Nouvelles Études*,’ and the trigonometrical survey of the same glacier executed by M. Wild under his auspices, were works of unrivalled excellence when published, and have never been surpassed. And in the second place, without detracting in any way from the merit due to others, it may with certainty be affirmed that Professor Agassiz was the first to take the proper means to ascertain the relative motion of the central and lateral parts of a glacier, which was done in the autumn of 1841; and that he also had the good fortune to reap the fruit of his arrangements, by being the first to make public (in the ‘*Comptes Rendus*’ for August the 29th, 1842) the novel and to him unexpected result that the centre of a glacier moves faster than the sides.

PROFESSOR MILLER,

It is the happy combination of intellectual genius with diligence and perseverance that has enabled M. Agassiz to arrive at the great results, a brief statement of which has just been read by the Secretary.

2 L 2

In your hands, as Foreign Secretary, I place the Copley Medal which the Council have awarded to M. Agassiz, requesting you that, when you have the opportunity of transmitting it to him, you will at the same time express that as it is the greatest honour the Royal Society has to bestow, so it sufficiently shows the high estimation in which they hold his scientific labours.

A Royal Medal has been adjudicated to Dr. William B. Carpenter, F.R.S., for his Researches on the Foraminifera, contained in four memoirs in the 'Philosophical Transactions,' his Investigations into the Structure of Shell, his Observations on the Embryonic Development of Purpura, and his various other writings on Physiology and Comparative Anatomy.

Dr. Carpenter has long held a high place as a systematic writer on Human and Comparative Physiology, and his well-known works have served, more perhaps than any others of their time, to spread the knowledge of those sciences and promote their study among a large class of readers. These writings, moreover, while they admirably fulfil their purpose as systematic expositions of the current state of knowledge on the subjects which they comprehend, afford evidence throughout of much depth and extent of original thought on most of the great questions of Physiology.

While not unmindful, however, of these merits on the part of Dr. Carpenter, or of his earlier special contributions to science, the Council have awarded him the Medal, in accordance with the existing terms of its adjudication, on account of his researches on various branches of Comparative Anatomy and Physiology published in later years.

Among those researches which more especially demand notice on this occasion, the first in point of time is the series of elaborate investigations on the intimate structure of Shell. By these inquiries Dr. Carpenter discovered that a very definite structural arrangement exists in the shells of many mollusca, and presents modifications which serve, in many instances, to characterize natural groups, as being in harmony with the general affinities of the animal. The group of Brachiopoda, in particular, he showed to be thus distinguishable from other bivalves; and he further found that, among the Brachiopoda themselves, certain groups of species are differentiated

from the rest by having their shells perforated with large canals, occupied in the living animal by cæcal prolongations of the mantle. The presence of these perforations, which had previously been considered to be mere surface markings, Dr. Carpenter showed to be a constant character of the Terebratulidæ, and their absence an equally constant character of the Rhynchonellidæ; whilst, in other families, certain genera or subgenera are distinguished by their presence from those in which they are absent. The validity of this distinctive character has since been amply confirmed by Mr. Davidson in his elaborate investigations of British Fossil Brachiopoda.

Dr. Carpenter's first contribution to the minute study of the Foraminifera was a memoir read before the Geological Society in 1850, in which he showed the necessity of a careful microscopic inquiry into the structure of the organisms of the class in question, for the elucidation of their real nature and affinities; and, taken in connection with Professor W. C. Williamson's previous memoirs on *Polystomella crispa*, this memoir may be considered as having laid the foundation for the truly scientific study of the Foraminifera, which has since been vigorously prosecuted by Professor Williamson, MM. D'Archiac and Haime, and Messrs W. H. Parker and Rupert Jones, as well as by Dr Carpenter himself.

In the four memoirs on the minute structure of the most highly developed forms of this class which Dr. Carpenter has contributed to the 'Philosophical Transactions,' he has described some most remarkable types which were previously quite unknown; he has given a detailed account of the very complex organization existing alike in the foregoing and in types previously well known by external configuration; he has demonstrated the entire fallacy of the artificial system of classification hitherto in vogue, the primary divisions of which are based on the plan of growth; he has laid the foundation of a natural system, based on those characters in the internal structure and conformation of the shell which are most closely related to the physiological conditions of the animal; and, finally, by the comparison of very large numbers of individuals, he has proved the existence of an extremely wide range of variation among the leading types of Foraminifera; often reassembling under a single species, varying forms, which, for want of a sufficiently careful study, have been not merely separated into distinct species, but had been arranged under

different genera, families, and even orders. In this last conclusion, which has an important bearing upon the question of the real value and meaning of natural history species generally, Dr. Carpenter has been fully borne out by the parallel inquiries of Messrs. Parker and Rupert Jones, which, relating to an extensive series of less developed types not especially studied by him, form, as it were, the complement of his own.

In his researches on the embryonic development of *Purpura Lapillus*, Dr. Carpenter's attention was specially directed to the elucidation of the fact, that, from the many hundreds of egg-like bodies contained in each nidamental capsule, only about thirty embryos are evolved, each of them many times larger than the ovum from which it originated. It had been affirmed by some previous observers that the entire assemblage of ova coalesces into a single mass, which subsequently breaks up into a smaller number of portions, each of which develops itself into an embryo. Dr. Carpenter, on the other hand, was led to the conclusion, that of the total number of egg-like bodies, a few develop themselves into embryos after the usual plan of aquatic gasteropods, while the remainder coalesce into a single mass. To this mass the embryos attach themselves by their mouths, and gradually ingest the particles of which it is composed until it is all shared among them; they thus become distended to many times their original bulk, and on the additional store of nutriment thus obtained, their development is carried on to an advanced stage within the capsule. Dr. Carpenter's account of the process was warmly attacked by certain observers who had given a different explanation of it, but it was fully confirmed by subsequent trustworthy inquirers; and there seems a strong probability that it is true of the Pectinibranchiate Gasteropods generally, since in many of them the like replacement of numerous small egg-like bodies by a few large embryos has been observed.

#### DR. CARPENTER,

You have been long engaged in the cultivation of a science which, important as it is in various other ways, has this peculiar interest, that it leads us to a more exact knowledge than we could otherwise obtain of that part of the creation to which we ourselves belong; not only explaining the structure and functions of the various organs

of which the human body is composed, but even assisting us to obtain more accurate notions of those higher qualities, those intellectual and moral qualities, by which man is eminently distinguished from all other animals. In the name of the Royal Society I present to you this Medal as a token of their high appreciation of your labours.

A Royal Medal has been awarded to Mr. James Joseph Sylvester, F.R.S., for his *Memoirs and Researches in Mathematical Science*.

Professor Sylvester's mathematical writings extend over a period commencing in the year 1837; separately, and as a whole, they display in an eminent degree the originality and inventive and generalising power of their author, and they have very greatly contributed to the advance of pure mathematics, more particularly as regards the Finite Analysis or Algebra, in the widest sense of the word. Several of the earlier papers relate to subjects which are resumed and developed in those of the last ten years; and on this ground it is right to allude to the researches on the theory of determinants, and the dialytic method of elimination; and also to the remarkable discovery as to Sturm's Theorem. It is well known that the theorem in its original form gave only a *process* for finding the functions which determine the number and limits of the real roots of an equation; the determination of the actual expressions of these functions in terms of the roots was an extension and completion of the theory, the merit of which belongs exclusively to Professor Sylvester. The subject is considered in detail, and various new and valuable results in connexion therewith are obtained, in the elaborate memoir in the 'Philosophical Transactions' for 1853, "On a theory of the Syzygetic relations of two rational and integral functions, comprising an application to the theory of Sturm's functions and that of the greatest algebraical common measure." The same memoir contains also a very original theory of the intercalations or relative interpositions of the real roots of two independent algebraical equations, and a new method of finding superior and inferior limits to the roots of an equation, characterized by the employment of formulæ involving arbitrary coefficients which may be determined so as to bring the limits into coincidence with the extreme roots. The memoir contains also, in connexion with the subjects to which it primarily relates, valuable researches on the theory of Invariants.

In regard to this theory, several capital discoveries belong to Professor Sylvester,—the Law of Reciprocity considered as a law relating to the *number* of Invariants,—Contravariants, which, although now seen to be included in the notion of an invariant, were a conception to which is due much of the progress of the theory,—the theory of the Canonical forms of binary functions of an odd order, and (less completely developed) the more difficult theory for those of an even order,—and Combinants, a theory, the resources of which are still to be developed, but a first fruits of which was the determination, in a manageable form, of the resultant of three ternary quadratic functions.

Only a sketch of a singularly elegant geometrical theory of the derivative points of a cubic curve has as yet been published, in a paper in the 'Philosophical Magazine' (1858).

The very original investigations forming the subject of the Lectures on Partitions are also as yet published in an incomplete form.

There are many other papers which might with propriety be specially noticed, but it is obviously impossible on the present occasion to give anything like a complete account of the labours of Professor Sylvester; among the latest of them are the researches on the Involution of six lines. The nature of the relation can be easily explained. Six lines may be such that, considered as belonging to a rigid body, there exists forces acting along these lines which keep the body in equilibrium; or, what is the same relation between them, they may be such that the equilibrium of a system of forces about these lines as axes, does *not* imply the complete equilibrium of the system of forces. But the consideration of such a system of lines leads to a long series of geometrical theorems relating to curves in space, and ruled surfaces of the third and fourth orders, and opens a wide field for future researches.

#### PROFESSOR SYLVESTER,

Passing over the metaphysical question as to the origin of those simple conceptions from which as a starting-point all mathematical inquiries must set out, it is plain that whatever is done afterwards is the result of the exercise of the pure intellect; and there is perhaps nothing more remarkable in the history of human nature, or which tends to give us so exalted a notion of the powers of the human mind, as that out of such simple materials so marvellous a fabric should

have been created as that of modern mathematics. Your eminence as a mathematician is so universally acknowledged that it can need no eulogium from myself, and I have therefore only to add that the Medal which I now place in your hands, is awarded to you by the Royal Society as a just acknowledgment of the value of your labours.

On the motion of Sir R. Murchison, seconded by Sir H. Holland, it was resolved,—

“That the thanks of the Society be returned to the President for his Address, and that he be requested to allow it to be printed.

“That the Society feel deeply indebted to Sir Benjamin Brodie for the care he has continued unremittingly to bestow on the interests committed to him as President, and desire to record the expression of their sincere regret for his retirement, and for the cause which has led to it.”

The Statutes relating to the election of Council and Officers having been read, and Mr. John Bishop and Mr. John Lubbock having been, with the consent of the Society, nominated Scrutators, the votes of the Fellows present were collected.

The following Gentlemen were declared duly elected Council and Officers for the ensuing year :—

*President.*—Major-General Edward Sabine, R.A., D.C.L., LL.D.

*Treasurer.*—William Allen Miller, M.D., LL.D.

*Secretaries.*— { William Sharpey, M.D., LL.D.  
George Gabriel Stokes, Esq., M.A., D.C.L.

*Foreign Secretary.*—William Hallows Miller, Esq., M.A.

*Other Members of the Council.*—John Couch Adams, Esq., M.A., D.C.L.; Sir William George Armstrong, C.B.; Benjamin Guy Babington, M.D.; Sir Benjamin Collins Brodie, Bart., D.C.L.; George Bowdler Buckton, Esq.; William Benjamin Carpenter, M.D.; Sir Philip de Malpas G. Egerton, Bart.; William Fairbairn, LL.D.; Captain Douglas Galton, R.E.; Wm. Robert Grove, Esq., M.A., Q.C.; William Hopkins, Esq., M.A., LL.D.; John Lubbock, Esq.; James Paget, Esq.; Joseph Prestwich, Esq.; William Spottiswoode, Esq., M.A.; John Tyndall, Esq.



# *Receipts and Payments of the Royal Society between November 30, 1860, and December 1, 1861.*

	£	s.	d.
Balance at Bank.....	£497	12	0
" of Catalogue Account .....	15	1	3
Subscriptions and Compositions .....	1638	12	0
Rents .....	236	9	6
Dividends on Stock (inclusive of Trust Funds) .....	1128	14	8
Sale of Transactions, Proceedings, &c. ....	295	9	6
Stevenson Bequest:—Dividends .....	494	0	9
Geological Society, Tea Expenses and Gas ...	£27	18	4
Linnean Society, ditto .....	15	18	9
Chemical Society, ditto .....	15	18	9
Geographical Society, Expenses for Gas .....	8	4	8
Parcel Charges recovered .....			
Miscellaneous .....	16	5	2
F. Ronalds, Balance repaid to Donation Fund .....	3	10	4
	20	5	0

*Estates and Property of the Royal Society, including Trust Funds.*

Estate at Mablethorpe, Lancashire (55 A. 2 R. 2 F.), £117 6s. 4d. per annum.

Estate at Acton, Middlesex (34 A. 3 R. 11 F.), £110 0s. 0d. per annum.

Free farm rent in Sussex, £19 4s. per annum.

One-fifth of the clear rent of an estate at Lambeth Hill, from the College of Physicians, £3 per annum.

£14,000 Reduced 3 per Cent. Annuities.

£26,477 8s. 0d. Consolidated Bank Annuities.

£513 9s. 8d. New 2½ per Cent. Stock.

*Scientific Relief Fund.*

Investments up to July 2nd, 1861, New 3 per Cent. Annuities, £4934 16s. 0d.

EDWARD SABINE,  
*Treasurer.*

£4414 0 8

	£	s.	d.
Salaries, Wages, and Pension .....	969	11	3
Gratuity of half-a-year's Salary to Mr. Weld on quitting his office .....	150	0	0
	288	4	4
The Scientific Catalogue .....			
Printing Trans., Part II. 1860, Part I. 1861...£458 17 10			
Printing Proceedings, Nos. 41-46 .....	183	0	4
Ditto Miscellaneous .....	89	4	7
Engraving and Lithography .....	238	15	0
Paper for Transactions and Proceedings ..	263	4	6
Binding and Stitching ditto .....	79	4	8
Books for the Library and Binding .....			
Donation Fund .....	306	0	2
Coal and Lighting .....	135	0	0
Painting, Cleaning, and Miscellaneous House Expenses ..	118	0	10
Tea Expenses .....	92	19	2
Fire Insurance .....	70	12	7
Law Expenses .....	42	1	6
Wimborneham Fund .....	34	19	0
Furniture and Repairs .....	34	5	6
Shipping Expenses .....	29	2	4
Taxes .....	17	11	6
Stationery .....	12	3	6
Copley Medal Fund .....	7	6	0
Rumford Medal Fund .....	5	19	0
Dr. Tyndall, Bakerian Lecture .....	3	18	0
Dr. Brown-Sequard, Croonian Lecture .....	3	18	4
Rev. T. S. Evans, Fairchild Lecture .....	2	17	6
Subscription, Mablethorpe Schools .....	2	17	2
Postage, Parcels and Petty Charges .....	2	2	0
Balance at Bank .....	57	8	0½
Library Account .....	£611	0	3
" Catalogue " .....	40	16	11
" Petty Cash " .....	26	16	11
	6	2	11½
	694	17	0½
	£4414	0	8

*Statement of Income and Expenditure (apart from Trust Funds) during the Year ending November 30, 1861.*

	£	s.	d.		£	s.	d.
Annual Subscriptions .....	£1138	12	0	Salaries, Wages, and Pension .....	969	11	3
Admission Fees .....	180	0	0	Gratuity of half-a-year's Salary to Mr. Weld on quitting his office .....	150	0	0
Compositions .....	320	0	0	The Scientific Catalogue .....	288	4	4
Rents .....	226	9	6	Printing Trans., Part II, 1860, Part I, 1861... £468 17 10			
Dividends on Stock (exclusive of Trust Funds) .....	849	4	1	Ditto Proceedings, Nos. 41-46 .....	183	0	4
" on Stevenson Bequest .....	494	0	9	Ditto Miscellaneous .....	89	4	7
Sale of Transactions, Proceedings, &c. ....	295	9	6	Engraving and Lithography .....	238	15	0
Geological Society, Tea Expenses and Gas £27 18 4				Paper for Transactions and Proceedings .....	293	4	6
Linnean Society, ditto .....	68	0	6	Binding and Stitching ditto .....	79	4	8
Chemical Society, ditto .....				Books for the Library .....	213	19	3
Geographical Society, Expenses for Gas .. 8 4 8				Binding ditto .....	92	0	11
Parcel Charges recovered .....	16	5	2	Coal and Lighting .....	306	0	2
Miscellaneous .....	3	10	4	Painting, Cleaning, and Miscellaneous House Expenses .....	118	0	10
	3601	11	10	Tea Expenses .....	92	19	2
				Fire Insurance .....	70	12	7
<i>Received too late to be included in the above.</i>				Law Expenses .....	42	1	6
Half Year's Dividend on £3452 1s. 1d., less Income }				Furniture and Repairs .....	34	19	0
Tax due in July 1861..... }	49	16	9	Shipping Expenses .....	29	2	4
Chemical Society, for Proceedings .....	50	0	0	Taxes .....	17	11	6
				Stationary .....	12	2	6
Income available for the Year ending Nov. 30, 1861 .....	3701	8	7	Subscription—Mablethorpe Schools .....	7	6	0
Expenditure in the Year ending Nov. 30, 1861 .....	3540	8	1½	Postage, Parcels, and Petty Charges .....	2	2	0
Excess of Income over Expenditure in the Year ending } Nov. 30, 1861 .....	£161	0	5½		57	8	0½
				EDWARD SABINE, Treasurer.	£3540	8	1½

The following Table shows the progress and present state of the Society with respect to the number of Fellows :—

	Patron and Honorary.	Foreign.	Having com- pounded.	Paying £2 12s. annually.	Paying £4 annually.	Total.
December 1, 1860..	7	49	338	7	272	673
Since compounded..	.....	.....	+1	.....	—1	.....
Since elected .....	.....	.....	+6	.....	+10	+16
Since deceased ....	—1	—1	—19	—2	—9	—32
Withdrawn .....	.....	.....	.....	.....	.....	.....
Defaulter .....	.....	.....	.....	.....	.....	.....
November 30, 1861	6	48	326	5	272	657

*December 5, 1861.*

Major-General SABINE, R.A., President, in the Chair.

The President stated that Mr. William Robert Sievier, who was readmitted into the Society on the 17th January, 1856, after having ceased to be a Fellow through default of payment of his annual contribution, had from a similar cause again ceased to be a Fellow in November 1860, and had applied for readmission. Mr. Sievier's letter to the Council was read, explaining the circumstances under which, during his absence on the Continent, the omission of payment had taken place. The Statute applying to the case was also read, and in accordance therewith notice was given that the question of Mr. Sievier's readmission would be put to the vote at the next Meeting.

Mr. Charles Spence Bate was admitted into the Society.

The President announced that he had appointed the following Members of the Council to be Vice-Presidents :

The Treasurer.

Sir Benjamin Collins Brodie, Bart.

William Robert Grove, Esq.

William Hopkins, Esq.

James Paget, Esq.

The following communications were read :—

- I. "On Crystallization and Liquefaction, as influenced by Stresses tending to change of form in the Crystals." By Professor JAMES THOMSON, Queen's College, Belfast. Communicated by Professor WILLIAM THOMSON. Received October 12, 1861.

In a paper submitted to the Royal Society, and printed in the 'Proceedings' for April 25th, 1861, I directed attention in a note (page 201), to the question of how the *surface* of a bar of ice in ice-cold water, as distinguished from the interior of the bar, may, by the application of tension to the bar, be influenced in respect to tendency either to melt away, or to solidify to itself additional ice from the water; but did not then venture to offer a positive answer. I pointed out as a matter deserving of special attention, and as affording scope for much additional theoretical and experimental investigation, the distinction between the application to ice in ice-cold water, of stresses tending to change its form, the stresses not being participated in by the water; and the application directly to the water, and through that to the ice, of cubical or hydrostatic pressures or tensions, these being participated in by the water and the ice alike; and I pointed out that the theory and quantitative calculation which I had originally given\* of the effect of pressure in lowering the freezing-point of water, or of diminution of pressure in raising it, applied solely to effects of pressure communicated to the ice *through the water*, and therefore equal in all directions, and equally occurring in the ice and the water; but that when changes of pressure in one or more direc-

\* Transactions Roy. Soc. Edin. vol. xvi. part 5, 1849; and Cambridge and Dublin Math. Journ. Nov. 1850.

tions are applied to the ice as distinguished from the water, the theory does not apply in any precise way to determine the conditions of the melting of the ice, or of its growth by the freezing of the adjacent water to its surface ; and I expressed the hope that I might subsequently communicate to the Society some further developments of the subject.

On following up various considerations which had then occurred to me, I soon formed positively the opinion that *any stresses whatever, tending to change the form of a piece of ice in ice-cold water* (whether these stresses be of the nature of pressures or tensions, that is pushes or pulls, and whether they be in one direction alone, or in more directions than one), *must impart to the ice a tendency to melt away, and to give out its cold, which will tend to generate, from the surrounding water, an equivalent quantity of ice free from the applied stresses.* I came also to the more general inference that *stresses tending to change the form of any crystals in the saturated solutions from which they have been crystallized must give them a tendency to dissolve away, and to generate, in substitution for themselves, other crystals free from the applied stresses or any equivalent stresses.* In the month of May last, I tested this inference by applying stresses to crystals of common salt in water saturated with salt dissolved from the crystals themselves ; and found the crystals to give way gradually, with a plastic yielding, like the yielding of wet snow, but very much slower. The crystals, with the brine in which they were immersed, were, in the first set of experiments, placed in a glass tube, like a test-tube, and a glass piston, or rammer, fitting the tube loosely, so as not to be water-tight, was placed on the top of the salt which lay like fine sand in the bottom, and the piston was loaded with weights. The piston went on descending from day to day through spaces, which, though small, and though diminishing as the crystals became more compacted against one another, were still distinctly visible. When the rate of descent became very slow, I added more weights, and found that the rate of descent increased, as was to be expected. I afterwards procured a strong brass cylinder with a loosely fitted, not water-tight piston, or rammer, and in this I subjected crystals of common salt in their saturated brine to very heavy stresses, and thus compressed them rapidly and easily into a hard mass like rock-salt. The top surface

presented a perfect impression of the tool marks on the bottom of the piston, such as might have been made in wax. The expulsion of the minute quantities of brine remaining in pores in the salt when it has become very closely compacted, appears to be a slow and difficult process ; as, after the pressure had been continued for about a fortnight, I still found a slight oozing of brine from a pore which happened to exist in the side of the cylinder.

Experiments by the application of tensile stresses, or of any other stresses than those mixed and chiefly compressive ones which arise when the crystals are pressed in a close vessel by a rammer, would probably not be very easily carried out ; and I have not as yet tried any except those by pressure. I feel quite convinced, however, that melting, or dissolving, must result from all kinds of stresses tending to change of form. I think the following statement may be assumed as a general physico-mechanical principle or axiom, and I think it involves the truth of the opinion just expressed :—

If any substance, or any system of substances, be in a condition in which it is free to change its state (whether of molecular arrangement, or of mechanical relative position and connexion of its parts, or of rest or motion), and if mechanical work be applied to it (or put into it) as potential energy, in such a way as that the occurrence of the change of state will make it lose (or enable it to lose) (or be accompanied by its losing) that mechanical work from the condition of potential energy, without receiving other potential energy as an equivalent ; *then the substance or system will pass into the changed state.* The consideration of a few cases, in some of which there is not freedom for the substance or system to change its state, and in others of which there is freedom, will render the meaning of this more clear.

Gunpowder may be cited as an example of a substance in a condition *not free to change its state*, although when it is made to explode by a spark, it passes to an altered condition, and, in doing so, even gives out a great amount of mechanical work. That is to say, that *on the whole* it is more than free to change to the exploded state, or it tends so to change, but there is some kind of obstacle at ordinary temperatures, to the change, which either vanishes at a high temperature, or requires the application of mechanical work to begin the over-

coming of it. When the change is once begun, the requisite help is given to the succeeding parts by those which have gone off first.

Again, water confined in a high reservoir is not free to go to a lower one ; although a siphon, primarily filled with water, may help the parts successively over the obstacle by lending to each the requisite mechanical work in advance, which it afterwards pays to the parts which are to follow, besides that it gives out in its fall a great additional amount of power or energy applicable otherwise. Two reservoirs of water, on the same level, and having an opening between them under the water surface, would represent the case of *perfect freedom for change of state* ; and two on a level with one another, but separated by a partition, would represent the case in which no mechanical work would finally be either given out or absorbed by the change, but in which there is not perfect freedom to change, until a siphon or other means of help is applied.

A bell hung from an axle and then turned up, and left resting against a stop a little beyond its position of unstable equilibrium, is not free to go down, but a slight pull will bring it over this position and make it free to swing, which the work stored as potential energy in the raising of it from its low or hanging position, will cause it to do ; its fall till it comes to the bottom being essentially accompanied by the loss of that potential energy, as such, though not as actual energy, out of the system of which it and the earth are the two parts, and in which change of their distance asunder constitutes change of their potential energy.

If in an atmosphere of steam resting on water at its boiling temperature for the pressure of the steam ; as, for instance, in the inside of a boiler partly filled with water, and partly with steam, an inverted cup, or bell-shaped vessel, be suspended, and if it then, being full of steam, be forced down under the water, mechanical work will be imparted as potential energy to the system of which the steam and water in the boiler form one part, and the earth is the other part ; though, for brevity of expression, the work may be spoken of as applied to the steam and water. In this case there is perfect freedom for the steam forced under the water to condense and cause by communication of its latent heat the generation of an equal quantity of steam at the surface of the water under which the bell was sunk.

The occurrence of this change of state will enable the system to lose the potential energy which had been imparted to it by the submergence of the steam, or will release that energy which had been stored, and the system *will pass into the changed state*; that is to say, a certain part of the steam will change to water, and, instead of that, a different part of the water will be changed to steam; and this change will be accompanied by a transmission of heat from the part condensing to the part evaporating. This is all in accordance with the axiom; and we know otherwise that it must take place, as the steam being pressed when submerged must condense and give its latent heat to the water, and that heat must generate an equal quantity of steam at the surface of the water, where the pressure is less. Thus the truth of the axiom is confirmed.

If a quantity of ice and water be enclosed in a cylinder with a water-tight piston, and if this be put into a completely closed vessel filled with other ice and water, and if the piston then be pulled with any given force and fixed in its new position (which might be done in many ways, as for instance, by the use of an axle passing air-tight through the side of the outer vessel), mechanical work will be introduced as potential energy into the system consisting of all the things enclosed in the outer vessel. But there is perfect freedom for the water enclosed in the cylinder to proceed to freeze, obtaining the requisite cold from the ice in the water confined around the cylinder and within the outer vessel. The occurrence of this change would be accompanied by the system's losing or giving up the potential energy which had been stored in it. According to the axiom, then, the change ought to occur. But we know otherwise that it must occur; because the diminution of hydrostatic pressure in the cylinder raises the freezing-point of the enclosed water, and makes it freeze by the cold of the surrounding mixture of ice and water, which, besides, by being itself subjected to increased pressure, tends to give out cold by the lowering of its freezing-point. Thus the truth of the axiom is again confirmed.

Lastly, if a bar of ice in ice-cold water be subjected to any stress (a pull for instance) tending to change its form, it will receive mechanical work from the force, or forces, applied, and that work will be stored as potential energy in the elasticity of the ice. Now, if there be another piece of ice in juxtaposition with this piece, seeing



that, at the beginning, both these pieces were free from externally applied forces, and were both in the state in which either was perfectly free to melt and cause an equal quantity of water to freeze to the other\*, it will follow according to the axiom, now supposed to be established, that the application of the stress *will cause this action to occur*.

The case of crystals in their solutions might be stated almost in the same words as the case of ice in ice-cold water : but it is to be observed that, in their case, the necessity for the translation of one chemical substance through another (the salt through the dissolving liquid), and not of heat or cold alone, causes a great slowness of the process, as compared with that of the yielding of the ice, in ice-cold water, to applied stresses.

At an early stage of the considerations which led to the opinions on the influence of stresses on crystallisation and liquefaction described in the present paper, the question arose to me :—Is a spiculum or single crystal of ice, which has solidified itself in the interior of water, and is therefore not colder than the water, plastic ? Or would it, when in the water, and attached by one end, as for instance to a crust of ice lining the containing vessel, gradually bend upwards by its own bouyancy in the heavier water ? My idea is that it is not plastic. I cannot conceive of the growth of a crystal proceeding with one continuous or uninterrupted structural arrangement, if during its growth

\* The supposition here assumed, however, of there being perfect freedom for either of two pieces of ice, which are immersed in the same water, and are alike free from stresses, to melt, and, by giving out its cold, to cause an equal quantity of water to freeze to the other, will probably not meet with assent at present from all, as it appears to be a prevailing opinion that water and ice in contact are *not* in a state of perfect indifference as to retaining or interchanging their conditions. It is supposed that ice has a property of tending to solidify water in contact with it, and the more so if there be ice on both sides of the water than if on only one side. Again, it is supposed that ice is essentially colder than water in contact with it, and that the water must continually be giving off heat to the ice. Both these opinions are inconsistent with the supposition here assumed. I conceive, however, that that supposition is amply confirmed by the fact that it was involved essentially throughout the reasoning, by which I was led to conclude that the freezing-point is lowered by increase of pressure, and to calculate the amount of the lowering. That reasoning led to true results and I believe it could not have done so unless the supposition were true, that when water and ice are present together their freedom to change their state on the slightest addition or abstraction of heat, or on the slightest application of mechanical work tending to the change, is perfect.

the part already formed undergoes permanent change of form, such as would be due to any plastic or ductile yielding. I think we must suppose the molecules in the interior of one crystal to be so locked into one another, by the forces of crystalline cohesion, that any one of them, or set of them, would experience a difficulty in making a beginning of the change of state from solid to liquid. I have not succeeded even in forming any clear conception of continuous crystalline structure admitting of what may be called ductile or malleable bending (that is, bending beyond limits of elasticity such as occurs in lead, copper, tin, and many other metals), and still remaining of the nature of one continuous crystal. What in soft or malleable crystals of copper or other metals, deposited in the electrotype process, may be the nature of the change of molecular arrangement induced by bending them, I cannot say; but I suppose that, in their yielding, their crystalline structure is materially altered, and rendered discontinuous where, before, it was continuous.

In a mass of plastic ice, I incline to think that the internal melting, to which I attribute the plasticity, must occur at the surfaces of junction of separate crystals or fragments of crystals; though probably pores formed by melting, by pressures, or by stresses, may penetrate crystals by entering them from their moistened surfaces or their junctions with other crystals. It now becomes clear, I think, that the influence of stresses affecting the ice, and tending to make it melt without there being necessarily any consequent pressure applied to the water in contact with the ice, must come to be taken into account in any theory of the plasticity of ice approaching to completeness. This view does not, however, I think, supersede the theory of the plasticity of ice sketched out by myself in former papers, but rather constitutes an amendment, and further development of it. Any complete theory of the plasticity of ice, and of the nature of glacier motion, must comprise the conditions as to fluid pressure and structural arrangement of the water and air included in the ice, and must so explain the lamination of the glacier, seen as blue and white veins. My brother, Professor William Thomson, in papers in the 'Proceedings of the Royal Society' for February 25 and April 22, 1858, endeavoured to follow up my previously published views on the plasticity of ice with an explanation of the laminated structure, based on the same principles. The explanation he then offered, I

think, cannot fail to assist in suggesting the direction in which the true solution is ultimately to be sought for; yet I feel confident that no full and true solution has as yet been found\*.

In the foregoing part of the present paper, I have shown reason why stresses applied to crystals when in contact with the liquid from which they have been produced, should be expected to cause them to melt or dissolve away. The following line of reasoning to show that stresses applied to a crystal will cause a resistance to the deposition of additions to it from the liquid, or, in other words, a resistance to its growth, will, I think, prove to be correct. When a crystal grows, the additions, it seems to me, must lay themselves down in a state of molecular fitting, or regular interlocking with the parts on which they apply themselves; or, in other words, they must lay themselves down so as to form one continuous crystalline structure with the parts already crystallized. It thus seems to me that, if a crystal grows when under a stress, the new crystalline matter must deposit itself in the same state of stress as the part is in on which it lays itself. If, then, we consider a spiculum of ice growing in water, and if we apply any stress, a pull for instance, to it while it is thin, and then fix it in its distended state, and if then by the transference† to the water beside it of cold taken from any other ice at the freezing-point we cause it to grow, which it may do if there be no other crystal of ice beside it more free than it to receive accessions, then the additional matter will, I think, lay itself down in the same state of tensile stress as the original spiculum was put into by the applied pull. The contractile force of the crystal will thus be increased in proportion to the increase of its cross sectional area. If it now be allowed to contract and relax itself, it will give out, in doing so, more mechanical work than was applied to the original spiculum during distention. Hence there would be a gain of mechanical work with-

\* I have my brother's authority for stating that, although he believes the physical principles suggested in his papers here referred to to be capable of being developed into a true explanation of the phenomena, yet he considers further investigation necessary, and does not feel confident as to the correctness of that part of the explanation he offered, in which the mutual action of two vesicles in a line oblique to that of maximum pressure is considered.

† A theoretic air-engine for making such transferences of heat or cold was used in the reasoning by which I determined theoretically the lowering of the freezing-point by pressure; and the same is admissible here.

out any corresponding expenditure; or we could theoretically have a means of perpetually obtaining mechanical work out of nothing, unless it were the case that greater cold is required to freeze water into ice on the stressed crystal than on a crystal free from stress. Hence we must suppose that a greater degree of cold will be required to cause the stressed crystal to grow. The reasoning just given has been for brevity stated somewhat in outline; but I trust the full meaning can readily be made out, and that what has been said may suffice.

I wish now to suggest as an important subject for investigation, The Effect of Change of Pressure (hydraulic pressure) in changing the Crystallizing Temperatures of Saline or other Solutions of given Strengths,—as I feel sure that such effect must exist, but am not aware that it has been hitherto discussed or experimented on, and as it is intimately connected with the matters under consideration in the present paper and with subjects discussed in previous papers, which I have submitted to the Royal Society, on Ice.

II. "Determination of the Magnetic Declination, Dip, and Force, at the Fiji Islands, in 1860 and 1861." By Colonel WILLIAM JAMES SMYTHE, of the Royal Artillery. Communicated by General SABINE, P.R.S. Received October 23, 1861.

[Note by the Communicator.—Colonel Smythe is known to magneticians as having been Director of the Magnetic Observatory at St. Helena from 1842 to 1847. Being about to proceed, in December 1859, on a Government Mission to the Fiji Islands, which would require his residence there for some months, he addressed a letter to the Council of the Royal Society expressing his readiness to make any scientific observations that might be suggested to him as likely to be useful in a part of the globe hitherto so little known. The Council directed that the Committee of the Kew Observatory should be informed of the opportunity thus offered of obtaining a reliable determination of the present values of the magnetic elements at the Fiji Islands; and Colonel Smythe was in consequence supplied with the necessary instruments from that establishment.

In communicating to the Society this paper, containing the results

of Colonel Smythe's observations, Major-General Sabine is desirous of drawing the attention of the Fellows to the thoroughly business-like manner in which Colonel Smythe has performed this useful service and to the illustration which it affords of the advantages anticipated by M. Gauss from the establishment of the British Colonial Magnetic Observatories—that "they would become schools for many good observers who would subsequently extend their activity over a wider range, and would contribute to arouse, to nourish, and to extend to other parts of natural knowledge that desire for the greatest possible accuracy in observation which was formerly met with only in Astronomy and in the higher Geodesy." (Letter to Sir John Herschel printed in the Reports of the British Association for the Advancement of Science for 1845, p. 45.)]

The accompanying observations were taken at a wooden house erected for the purpose on a clay soil 106 feet above the mean level of the sea, and distant 265 yards West ( $19^{\circ} 07' S.$ ) from the spot on the beach where I have been informed Captain Denham's observations were made. The latter spot is on volcanic rock thinly covered with sand and grass, close to the base of the low rocky promontory on which Commodore Wilkes, U.S.N., set up his Observatory.

*Declination.*—The values of this element, deduced from A.M. and P.M. observations of the sun's azimuth, differ by about  $10'$ . As the several determinations in each position agree very well, the difference is treated as instrumental error, and the mean of the two results taken as the true value of the Declination.

*Inclination.*—The individual readings of the needles in the various positions differed considerably. The means, however, accord.

*Intensity of the Force.*—By comparison of the time of vibration in December 1860 and April 1861, the suspended magnet is perceived to have lost force. During the interval, it remained in its box perfectly undisturbed. The value of  $\pi^2 K$  used in all the calculations is the same, as there was no means of ascertaining its temperature corrections. It was deduced from a series of vibrations with and without the gun-metal cylinder No. 6, in December 1860, after the conclusion of the observations for Intensity. The correction  $1 - \frac{P}{r_0^2}$  has not been applied.

**Magnetic Declination.**  
**Levuka, Fiji Islands. Lat. 17° 41' S. Long. 178° 52' E.**

Date.	☉ Altitude corrected.	☉ True Azimuth.	☉ Azimuth by Compass.	Magnetic Declination.
<div style="text-align: center;"> <div>1860.</div> <div>Observations.</div> <div>A.M.</div> </div>	d h m			
	Dec. 18 19 15	108 22	98 56	9 26 E.
	20 19 00	108 56	99 32	9 24
	20 19 30	107 42	98 16	9 26
	23 19 00	108 52	99 29	9 23
	25 19 15	108 17	98 52	9 25
	26 19 10	108 28	99 04	9 24
	27 18 45	109 33	100 06	9 27
				9° 25' 0
<div style="text-align: center;"> <div>1861.</div> <div>Observations.</div> <div>P.M.</div> </div>	Jan. 18 04 55	105 17	114 33	9 16 E.
	18 05 05	105 56	115 12	9 16
	24 05 05	104 31	113 46	9 15
	25 05 00	103 57	113 12	9 15
	25 05 10	104 19	113 35	9 16
	Feb. 18 04 40	94 55	104 10	9 15
	18 04 55	95 32	104 47	9 15
				9° 15' 4
				Mean Magnetic Declination 9° 20' 2 E.

Magnetic Inclination. Levuka, Fiji Islands. Lat.  $17^{\circ} 41' S$ . Long.  $178^{\circ} 52' E$ .

Date.	Needle.	Azimuth.	Poles direct.	Poles reversed.	Mean.	Dip deduced.
1861. Jan. 7	No. 1	Magnetic meridian.	$36^{\circ} 13' 44''$	$35^{\circ} 50' 25''$	$36^{\circ} 01' 8''$	$36^{\circ} 01' 80''$
8	"	"	$36^{\circ} 09' 25''$	$35^{\circ} 51' 25''$	$36^{\circ} 00' 25''$	$36^{\circ} 00' 25''$
13	"	"	$36^{\circ} 11' 40''$	$35^{\circ} 45' 60''$	$35^{\circ} 58' 50''$	$35^{\circ} 58' 50''$
22	"	"	$36^{\circ} 07' 00''$	$35^{\circ} 44' 25''$	$35^{\circ} 55' 62''$	$35^{\circ} 55' 62''^*$
23	"	"	$36^{\circ} 14' 00''$	$35^{\circ} 49' 81''$	$36^{\circ} 01' 90''$	$36^{\circ} 01' 90''$
Mar. 17	"	"	$36^{\circ} 17' 40''$	$35^{\circ} 36' 56''$	$35^{\circ} 57' 00''$	$35^{\circ} 57' 00''$
19 22 <sup>h</sup> to } 20 01 }	"	$45^{\circ} E$ . of Magnetic meridian at right angles to this position.	$46^{\circ} 11' 81''$ $46^{\circ} 11' 69''$	$45^{\circ} 36' 31''$ $45^{\circ} 35' 31''$	$45^{\circ} 54' 06''$ $45^{\circ} 53' 50''$	$36^{\circ} 06' 83''$
21 22 to } 22 01 }	"	$70^{\circ} E$ . of Magnetic meridian at right angles to this position.	$65^{\circ} 06' 62''$ $38^{\circ} 04' 19''$	$64^{\circ} 29' 62''$ $37^{\circ} 26' 40''$	$64^{\circ} 48' 12''$ $37^{\circ} 45' 30''$	$36^{\circ} 02' 43''$
Apr. 2 22 to } 3 01 }	"	$45^{\circ} W$ . of Magnetic meridian at right angles to this position.	$46^{\circ} 28' 44''$ $45^{\circ} 36' 62''$	$45^{\circ} 57' 69''$ $45^{\circ} 04' 75''$	$46^{\circ} 13' 06''$ $45^{\circ} 20' 68''$	$35^{\circ} 59' 48''$
3	"	Magnetic meridian.	$36^{\circ} 15' 62''$	$35^{\circ} 43' 94''$	$35^{\circ} 59' 78''$	$35^{\circ} 59' 78''$

Mean Magnetic Inclination by Needle No. 1 =  $36^{\circ} 00' 36''$

Jan. 8	0-01	No. 2	Magnetic meridian.	36 10'30	35 48'30	35 59'60	35 59'60
9	22-24	"	"	36 14'12	35 50'44	36 02'28	36 02'28
14	22-24	"	"	36 13'25	35 45'75	35 59'50	35 59'50
21	22-24	"	"	36 15'06	35 45'44	36 00'25	36 00'25
24	22-24	"	"	36 13'90	35 51'00	36 02'40	36 02'40
Mar. 18	22-24	"	"	36 17'75	35 42'75	36 00'25	36 00'25
20 22 <sup>h</sup> 10	21-24	"	45° E. of Magnetic meridian	46 04'75	45 23'00	45 43'87	35 56'89
21 01		"	at right angles to this position.	45 58'06	45 27'50	45 42'80	
22	21-24	"	70° E. of Magnetic meridian	64 50'31	64 12'75	64 31'53	36 01'01
		"	at right angles to this position.	38 00'00	37 32'94	37 46'47	
Apr. 1	21-24	"	45° W. of Magnetic meridian	46 16'00	45 50'94	46 03'47	35 58'56
		"	at right angles to this position.	45 46'31	45 07'87	45 27'09	
5	2-4	"	Magnetic meridian.	36 15'75	35 50'44	36 03'09	36 03'09
				Mean Magnetic Inclination by Needle No. 2 = 36° 00' 38			

\* This result was so much below the average that the observations were repeated, but without causing any change.

The Inclination is deduced from the observations made *out* of the Magnetic meridian by the formula  $\cos^2 \theta = \cos^2 \eta + \cos^2 \eta^1$ .



Intensity of the Magnetic Force. Levuka, Fiji Islands. Lat.  $17^{\circ} 41'$  S. Long.  $178^{\circ} 52'$  E.

Horizontal Component { Magnet III., deflecting.  
Temperature =  $20^{\circ}$ . Magnet S. III., suspended.

Date.	$r_0$	$w_r$	$\log \frac{m'}{X}$	$T_1$ log.	$\pi^2 K$	mX.	X.
1860. Dec. 8	1'0	6 36 32.5	8.76491	1.14513	1.67532	0.53019	7.6120
8	1'3	3 00 27.5	8.76548	"	"	"	7.6270
11	1'0	6 37 02.5	8.76552	1.14607	"	0.52925	7.6184
11	1'3	3 00 17.5	8.76515	"	"	"	7.6217
12	1'0	6 36 50.0	8.76504	1.14655	"	0.52877	7.6184
12	1'3	3 00 25.0	8.76511	1.14657	"	0.52875	7.6177
1861. Apr. 16	1'0	6 20 18.75	8.74666	1.16709	"	0.50823	7.5994
16	1'3	2 52 48.75	8.74632	"	"	"	7.6024
17	1'0	6 19 31.25	8.74517	1.16674	"	0.50858	7.6155
17	1'3	2 53 38.75	8.74553	"	"	"	7.6124
18	1'0	6 19 46.25	8.74509	1.16680	"	0.50852	7.6105
18	1'3	2 52 36.25	8.74534	"	"	"	7.6128
19	1'0	6 19 00.	8.74497	1.16650	"	0.50882	7.6194
19	1'3	2 52 22.5	8.74519	"	"	"	7.6175
Mean X =							7.6161

Total Magnetic Force = Horizontal Component  $\times$  sec. Inclination =  $7.6161 \times \sec. 36^{\circ} 00' 37'' = 9.4147$ .

III. "On the Calculus of Functions." By W. H. L. RUSSELL, Esq., A.B. Communicated by A. CAYLEY, F.R.S. Received October 31, 1861.

(Abstract.)

One of the first efforts toward the formation of the calculus of functions is due to Laplace, whose solution of the functional equation of the first order, by means of two equations in finite differences, is well known. Functional equations were afterwards treated systematically by Mr. Babbage; his memoirs were published in the Transactions of this Society, and there is some account of them in Professor Boole's Treatise on the Calculus of Finite Differences. A very important functional equation was solved by Poisson in his memoirs on Electricity; which suggested to me the investigations I have now the honour to lay before the Society.

I have commenced by discussing the linear functional equation of the first order with constant coefficients, where the subjects of the unknown functions are rational functions of the independent variable, and have shown how the solution of such equations may in a variety of cases be effected by series, or by definite integrals. I have then considered functional equations with constant coefficients of the higher orders, and have proved that they may be solved by methods similar to those used for equations of the first order. I have next proceeded with the solution of functional equations with variable coefficients. In connexion with functional equations, I have considered equations involving definite integrals, and containing an unknown function under the integral sign; the methods employed for their resolution depend chiefly upon the solution of functional equations, as effected in this paper. The calculus of functions has now for a long time engaged the attention of analysts; and I hope that the present investigations will be found to have extended its power and resources.

IV. "On Tschirnhausen's Transformation." By ARTHUR CAYLEY, Esq., F.R.S. Received November 7, 1861.

(Abstract.)

The memoir of M. Hermite, "Sur quelques théorèmes d'algèbre et la résolution de l'équation du quatrième degré," Comptes Rendus,

t. xlv. p. 961 (1858), contains a very important theorem in relation to Tschirnhausen's transformation of an equation  $f(x)=0$  into another of the same degree in  $y$ , by means of the substitution  $y=\phi x$ , where  $\phi x$  is a rational and integral function of  $x$ . In fact, considering for greater simplicity a quartic equation,

$$(a, b, c, d, e \chi x, 1)^4 = 0,$$

M. Hermite gives to the equation  $y=\phi x$  the following form,

$$y = aT + (ax+b)B + (ax^2+4bx+6c)C + (ax^3+4bx^2+6cx+d)D$$

(I write  $B, C, D$  in the place of his  $T_0, T_1, T_2$ ), and he shows that the transformed equation in  $y$  has the following property: viz., every function of the coefficients which, expressed as a function of  $a, b, c, d, e, T, B, C, D$ , does not contain  $T$ , is an *invariant*, that is, an invariant of the two quantics

$$(a, b, c, d, e \chi X, Y)^4, (B, C, D \chi Y, -X)^2.$$

This comes to saying that if  $T$  be so determined that in the equation for  $y$  the coefficient of the second term ( $y^2$ ) shall vanish, the other coefficients will be invariants; or if in the function of  $y$  which is equated to zero we consider  $y$  as an absolute constant, the function of  $y$  will be an invariant of the two quantics. It is easy to find the value of  $T$ ; this is in fact given by the equation

$$0 = aT + 3bB + 3cC + dD;$$

and we have thence for the value of  $y$ ,

$$y = (ax+b)B + (ax^2+4bx+3c)C + (ax^3+4bx^2+6cx+3d)D;$$

so that for this value of  $y$  the function of  $y$  which equated to zero gives the transformed equation will be an invariant of the two quantics. It is proper to notice that in the last-mentioned expression for  $y$ , all the coefficients except those of the term in  $x^2$ , or  $bB + 3cC + 3dD$  are those of the binomial  $(1, 1)^4$ , whereas the excepted coefficients are those of the binomial  $(1, 1)^3$ ; this suffices to show what the expression for  $y$  is in the general case.

I have in the two papers, "Note sur la transformation de Tschirnhausen" and "Deuxième Note sur la transformation de Tschirnhausen" (Crelle, t. lviii. pp. 259 and 263, 1861), obtained the transformed equations for the cubic and quartic equations; and by means of a grant from the Government Grant Fund, I have been enabled to procure the calculation by Messrs. Davis and Otter, under my

superintendence, of the transformed equation for the quintic equation. The several results are given in the present memoir; and for greater completeness, I reproduce the demonstration which I have given in the former of the above-mentioned two notes, of the general property, that the function of  $y$  is an invariant. At the end of the memoir I consider the problem of the reduction of the general quintic equation to Mr. Jerrard's form  $x^5 + ax + b = 0$ .

*December 12, 1861.*

Major-General SABINE, R.A., President, in the Chair.

In accordance with the announcement made from the Chair at the last Meeting, the question of Mr. Sievier's readmission was put to the vote, and was decided in the affirmative. The President accordingly declared that Mr. Sievier was readmitted into the Society.

The following communications were read :—

- I. "On a Series for calculating the Ratio of the Circumference of a Circle to its Diameter." By AMOS CLARKSON, Esq. Communicated by Professor STOKES, Sec. R.S. Received September 27, 1861.

The ratio ( $\pi$ ) of the circumference to the diameter of a circle may be calculated by the following series :—

$$\pi = \frac{8}{3} \left\{ 1 - \frac{1}{3 \cdot 10} - \frac{2}{3 \cdot 5 \cdot 10^3} - \frac{2 \cdot 4}{3 \cdot 5 \cdot 7 \cdot 10^5} - \dots \right\} \\ + \frac{4}{7} \left\{ 1 - \frac{2}{3 \cdot 10^2} - \frac{2 \cdot 2^2}{3 \cdot 5 \cdot 10^4} - \frac{2 \cdot 4 \cdot 2^3}{3 \cdot 5 \cdot 7 \cdot 10^6} - \dots \right\}. \quad (1)$$

This series may be thus established. We have, as is well known,

$$\frac{\pi}{4} = 2 \tan^{-1} \frac{1}{3} + \tan^{-1} \frac{1}{7};$$

and denoting by  $c$  the arc of which the tangent is  $t$ ,

$$c = t - \frac{t^3}{3} + \frac{t^5}{5} - \frac{t^7}{7} + \dots$$

Put

$$t^2 = \frac{1}{x-1},$$

then

$$c = \frac{1}{(x-1)^{\frac{1}{2}}} \left\{ 1 - \frac{1}{3(x-1)} + \frac{1}{5(x-1)^2} - \frac{1}{7(x-1)^3} + \dots \right\}$$

and

$$-\frac{1}{3(x-1)} = -\frac{1}{3} \left( \frac{1}{x} + \frac{1}{x^2} + \frac{1}{x^3} + \frac{1}{x^4} + \dots \right),$$

$$\frac{1}{5(x-1)^2} = \frac{1}{5} \left( \frac{1}{x^2} + \frac{2}{x^3} + \frac{3}{x^4} + \dots \right),$$

$$-\frac{1}{7(x-1)^3} = -\frac{1}{7} \left( \frac{1}{x^3} + \frac{3}{x^4} + \dots \right),$$

$$\frac{1}{9(x-1)^4} = \frac{1}{9} \left( \frac{1}{x^4} + \dots \right);$$

whence we get by addition,

$$c = \frac{1}{(x-1)} \left\{ 1 - \frac{1}{3x} - \frac{2}{3 \cdot 5x^2} - \frac{2 \cdot 4}{3 \cdot 5 \cdot 7x^3} - \dots \right\}. \quad (2)$$

The law of the coefficients may be discovered by induction, but is not easily demonstrated in this manner. It may be obtained as follows:—

We have by differentiation

$$dc = \frac{dt}{1+t^2} = -\frac{dx}{2x(x-1)^{\frac{1}{2}}}. \quad (3)$$

Assume

$$-\frac{dx}{2x(x-1)^{\frac{1}{2}}} = d \left\{ \frac{1}{(x-1)^{\frac{1}{2}}} \left[ A + \frac{B}{x} + \frac{C}{x^2} + \frac{D}{x^3} + \dots \right] \right\}; \quad (4)$$

then

$$-\frac{dx}{(x-1)^{\frac{1}{2}}} \left( \frac{x-1}{x} \right) = -\frac{dx}{(x-1)^{\frac{1}{2}}} \left\{ A + \frac{B}{x} + \frac{C}{x^2} + \frac{D}{x^3} + \dots + 2(x-1) \left( \frac{B}{x^2} + \frac{2C}{x^3} + \frac{3D}{x^4} + \dots \right) \right\}$$

or

$$1 - \frac{1}{x} = A + \frac{3B}{x} + \frac{5C}{x^2} + \frac{7D}{x^3} + \dots - \frac{2B}{x^2} - \frac{4C}{x^3} - \dots$$

and by equating coefficients,

$$A=1,$$

$$B=-\frac{1}{3},$$

$$C=\frac{2}{5}B=-\frac{2}{3.5},$$

$$D=\frac{4}{7}C=-\frac{2.4}{3.5.7}, \text{ \&c. ;}$$

whence substituting in (4) and (3), integrating, and observing that  $c=0$  when  $t=0$  or  $x=\infty$ , we have the series (2). On substituting now in succession  $\tan^{-1}\frac{1}{3}$  and  $\tan^{-1}\frac{1}{7}$  for  $c$ , and therefore 10 and 50 for  $x$ , and in the latter case multiplying the numerators and denominators of the successive terms by successive powers of 2, we obtain the series (1).

These series, which the author believes to be new, follow a simple law, and converge with great rapidity. But their distinctive feature, compared with other series which have been given for the same object, consists in the fact that the denominators involve the successive powers of 10, the division by which is effected at once.

## II. "On the Production of Vibrations and Sounds by Electrolysis." By GEORGE GORE, Esq. Communicated by Professor TYNDALL. Received November 12, 1861.

(Abstract.)

In this communication, which is a continuation in subject (but different in title) of a previous investigation "On the Movements of Liquid Metals and Electrolytes in the Voltaic Circuit," the author has described the most convenient and effective method of obtaining vibrations and sounds by electrolysis.

The paper contains a full account of the influence of various circumstances upon the vibrations and sounds: viz., of the electrodes,—the electrolyte,—mechanical circumstances and temperature,—the electric current,—size and number of voltaic elements,—quantity of the current,—coils of wire in the circuit,—induction coils and iron cores,—electrolytes in the circuit,—and of magnetism: also the influence of the vibrating medium itself upon the electric current.

The best liquid for producing the vibrations and sounds consists of

10 grains of cyanide of mercury and 100 grains of hydrate of potash dissolved in  $2\frac{3}{4}$  ounces of aqueous hydrocyanic acid containing 5 per cent. of anhydrous acid.

The vibrations and sounds occur only at the surface of the negative mercury electrode, as already described (Proc. Roy. Soc. No. 44, page 177); and out of a large number of liquids examined, the only ones in which *phonetic* vibrations occurred were solutions of alkaline cyanides containing dissolved mercury; and these only give the sounds with electrodes (or at least a cathode) of mercury, not with solid metals nor with fused alloys.

The vibrations and sounds vary considerably according to the size and number of the voltaic elements; with a few elements of large surface, the vibrations were small and the sounds high, and with many elements of small surface they were much larger and the tone lower. The most suitable number of elements to produce them is either two of Grove's or five of Smee's.

The interposition of a coil of stout copper wire in the circuit made the vibrations wider and the sound more base; and if an iron core was suddenly thrust into the axis of the coil, they became still wider and the sounds still more base, and remained so as long as the iron continued there; but if a secondary coil containing a great length of fine copper wire surrounded the primary coil (with or without an iron core), and the ends of the secondary wire were suddenly united, the vibrations instantly became narrower and the sounds more high, and remained so as long as the secondary circuit was closed.

The vibrations of the mercury and electrolyte make the electric current which produces them sensibly *intermittent*, similar to the influence of a vibrating coil-hammer; and they may be used to some extent in a similar way to that instrument to produce shocks, &c. by means of a secondary coil. A strong electro-magnet placed in various positions near the locality of the vibrations had no perceptible influence upon them.

The author considers the vibrations to be of *electro-chemical* origin, and to result from an attraction between the mercury of the negative electrode and the mercury of the electrolyte. He supposes that to produce the vibrations, either the voltaic force itself must be of an intermittent nature, or the resistance opposed to that force by the liquids employed must be intermittent, and intends to make the

experimental investigation of this question, with other allied matters, the subject of a future communication.

III. "On Perchloric Acid and its Hydrates." By HENRY ENFIELD ROSCOE, B.A., Ph.D., Professor of Chemistry in Owens College, Manchester. Communicated by Professor A. W. WILLIAMSON. Received December 12, 1861.

Stadion\* in the year 1816 showed that perchlorate of potassium contains 45.92 per cent. of oxygen, and that its composition is therefore represented by the formula  $\text{ClO}_4\text{K}$  (requiring 46.17 per cent. of oxygen). Mitscherlich† and Serullas‡ in 1830, and Marignac§ in 1841, confirmed this result, their experiments respectively showing that the salt in question contains 46.06, 46.20, and 46.17 per cent. of oxygen.

Upon these determinations is based nearly all the knowledge we possess of the quantitative relations of perchloric acid. The anhydride,  $\text{Cl}_2\text{O}_7$ , has not been isolated; no analyses of the hydrated acid itself have been published, and the composition of only one or two of its salts has been ascertained. Our acquaintance even with the general characters of this substance is also most limited; and we can only account for the neglect with which chemists have treated the highest and yet the most stable of the oxides of chlorine by the fact that the preparation of the acid in large quantity has hitherto been attended with great difficulties.

In the following communication I have to detail the results of experiments which I believe somewhat enlarge our views respecting the nature and properties of this interesting acid.

The first point to which attention was naturally directed, was the best mode of preparing the pure aqueous perchloric acid in quantity. After trial of a great number of methods, the following modification of that recommended by Serullas was adopted as yielding the best results. A large quantity of a saturated solution of hydrofluosilicic acid was prepared by heating dry sand and fluor-spar with more than the equivalent quantity of twice-rectified oil of vitriol in large stoneware bottles, and leading the gaseous fluoride of silicon with the usual

\* Gilbert's Annalen, lii. pp. 197 and 339.

† Pogg. Ann. xxv. p. 287.

‡ Ann. de Ch. et de Phys. xlv. p. 270.

§ Ann. Ch. Pharm. xlv. p. 11.



precautions into water. After straining off the precipitated silica, the acid was boiled down with chlorate of potassium (and not perchlorate, as Serullas advises), added in such quantity that for every kilogramme of fluor-spar 600 grammes of chlorate was present. On heating the mixture, the liberated chloric acid undergoes quiet decomposition, chloric oxide, and probably other oxygen compounds of chlorine, escaping as gas, whilst aqueous perchloric acid remains in the liquid. After the mixture has cooled, the aqueous acid was decanted from the insoluble fluosilicate of potassium, and the clear liquid concentrated by boiling until the perchloric acid began to volatilize. The acid was then distilled, and freed from chlorine by perchlorate of silver, and from sulphuric acid by perchlorate of barium. According to this method, 4 kilogs. of chlorate of potassium yielded 500 grammes of pure concentrated aqueous perchloric acid.

The aqueous acid thus prepared is a colourless, heavy, oily liquid, in appearance undistinguishable from oil of vitriol; its composition and properties will be hereafter described.

From this substance I have succeeded in preparing two definite compounds. One of these is the liquid pure perchloric acid,  $\text{ClO}_4\text{H}$ , an interesting body which was previously unknown; and the other is the monohydrated or crystallized acid,  $\text{ClO}_4\text{H} + \text{H}_2\text{O}$ , originally discovered by Serullas, but obtained by him in such small quantities that he was unable to determine its composition.

If aqueous perchloric acid be distilled with four times its volume of oil of vitriol containing 98.5 per cent. of real acid, decomposition begins at a temperature of about  $110^\circ\text{C}$ .; dense white fumes are evolved and escape, whilst a yellowish, mobile, easily condensable liquid is found in the receiver. If the distillation be continued, the temperature gradually rises to  $200^\circ\text{C}$ ., and a thick oily liquid distils over, which, on coming into contact with the more volatile liquid in the receiver, immediately unites with it, forming a white, solid, crystalline mass. Both the first and the last portions of the distillate thus prepared were found to contain sulphuric acid mechanically carried over, and were therefore unfit for analysis. In order to obtain the substances in a pure state, the crystallized acid thus prepared requires redistillation. Heated, however, to a temperature of  $110^\circ\text{C}$ ., the crystals themselves undergo a decomposition similar to that just described, a strongly fuming volatile liquid first distilling over, and the

oily acid appearing in the neck of the retort when the temperature rises to  $203^{\circ}\text{C.}$ , and producing crystals on coming in contact with the first distillate. These crystals are likewise formed when water is added to the volatile liquid ; and this is, in fact, the best mode of preparing them in the pure state.

### 1. *Perchloric Acid*, $\text{ClO}_4\text{H}$ .

Pure perchloric acid may be obtained in quantity by the process above described, or, more easily, according to the following method. Perchlorate of potassium is distilled together with four equivalents of strong sulphuric acid, until the drops of distillate no longer solidify on falling into the receiver ; in this way 100 grammes of perchlorate yield 14 grammes of crystallized perchloric acid containing traces of sulphuric acid. The impure crystals are then melted, placed in a small bulb-retort, and gently heated. Ebullition commences at  $110^{\circ}\text{C.}$ , and the decomposition continues for some time without further aid of heat ; dense white fumes, accompanied by small quantities of chlorine, issue from the mouth of the apparatus, and a mobile, easily condensable, heavy liquid is found in the receiver. This liquid is pure perchloric acid,  $\text{ClO}_4\text{H}$ , which, if the distillation has been conducted slowly, is perfectly colourless, but often has a slightly yellow colour, owing to the presence of lower oxides of chlorine. As soon as the first appearance of crystals in the neck of the retort is noticed, the distillation is stopped. The liquid distillate should then be at once sealed up in a bulb with long capillary ends, out of which any wished-for quantity may be obtained at pleasure. The greatest care is necessary in working with this body, owing to its explosive and corrosive properties.

The composition of the liquid thus prepared was determined in two ways : in the first place, by the estimation of the quantity of the potassium salt which a given weight of the acid yielded, as also of the quantity of chlorine, oxygen, and potassium which this salt contained ; and secondly, by estimation of the quantity of barium needed to form a neutral salt with a given weight of the acid. As the acid on exposure to the air absorbs moisture with the greatest avidity, producing the crystals, it is necessary that the acid for analysis should be quickly sealed up in weighed bulbs.

*Analysis No. 1.*—0.7840 grm. of pure perchloric acid, prepared as

2 N 2

last described, was mixed with water and neutralized by an excess of pure carbonate of potassium prepared from the tartrate. Acetic acid was then added to the acid reaction, and the whole evaporated to dryness on the water-bath. Treated with absolute alcohol, the insoluble\* perchlorate was thrown upon a weighed filter, and completely freed from soluble acetate by washing with absolute alcohol. The weight of perchlorate of potassium obtained was 1.080 grm.; hence the liquid contained 0.7837 grm. of  $\text{ClO}_4\text{H}$ , or 99.93 per cent. Of this potassium-salt, 0.9915 grm. lost, on heating in a long test-tube with pure peroxide of iron, 0.457 grm.; and the chlorine in the residual chloride of potassium required 0.7683 grm. of pure silver for complete precipitation.

*Analysis No. 2.*—A portion of acid prepared on another occasion gave the following results. Weight of acid taken 1.2185 grm.; weight of dry potassium-salt obtained 1.6785 grm.: of this salt 0.9660 grm., heated with peroxide of iron, lost 0.4440 grm., and the residual chloride required 0.7440 grm. of pure silver for complete precipitation. 0.3165 grm. of the same perchlorate yielded, on treatment with strong sulphuric acid, 0.2010 grm. of sulphate of potassium. Hence the liquid contained 1.2179 grm., or 99.95 per cent. of  $\text{ClO}_4\text{H}$ .

*Analysis No. 3.*—0.3155 grm. of pure acid was diluted with water and treated with an excess of freshly precipitated carbonate of barium. On filtering and washing, the barium in the filtrate was estimated as sulphate, of which salt 0.3660 grm. was obtained, showing that the substance contained 0.3157 grm., or 100.06 per cent. of  $\text{ClO}_4\text{H}$ .

*Composition of Potassium-Salt obtained from acid.*

			Found.	
			No. 1.	No. 2.
Cl .....	35.5	Calculated. 25.62	25.48	25.37
O <sub>4</sub> .....	64.0	46.17	46.09	46.24
H .....	39.0	28.21		28.51
	138.5	100.00		100.12

\* It was found that 2 grms. of pure perchlorate of potassium, after washing with 100 cub. cent. of absolute alcohol containing a trace of acetate in solution, and then with 50 cub. cent. of pure alcohol, lost  $\frac{1}{15}$  of a milligramme.

Perchloric acid is a heavy, colourless, volatile liquid, which does not solidify at temperatures above  $-35^{\circ}\text{C}$ . At  $15^{\circ}\cdot 5^{\circ}\text{C}$ . the specific gravity of the liquid was found to be 1.782 as a mean of two determinations. The vapour of perchloric acid is colourless and transparent; but when brought into contact with the air, dense masses of white fumes of hydrated acid are given off, owing to rapid absorption of atmospheric moisture. Perchloric acid does not give up its oxygen to metals at the ordinary temperature as readily as nitric acid; but when heated with finely divided iron, copper, zinc, &c., the oxides of these metals are formed, with evolution of light and heat; sulphur and phosphorus are likewise at once converted into their highest oxides in presence of this acid. It is, however, when brought into contact with a carbonized material that the energy of the oxidizing action of perchloric acid becomes visible, as a single drop of this substance falling upon charcoal, paper, or wood, causes an explosive combustion which in violence does not fall far short of the detonation produced by the sudden decomposition of the chloride of nitrogen. Dropped into ether or alcohol, an explosion occurs violent enough to break in pieces the vessel containing the liquid. This latter decomposition may possibly be owing to the formation of the explosive body, described by Messrs. Hare and Boyle as the perchlorate of ethyl. Perchloric acid likewise combines with the greatest energy with water, causing a loud hissing noise, and forming either the crystallized or the oily acid, according to the relative quantities of water and acid present. In working with this substance the greatest care must be taken, as one drop falling upon the skin produces powerful cauterization, and leaves a wound which does not heal for weeks. Like nitric acid,  $\text{NO}_3\text{H}$ , which in many respects it closely resembles, perchloric acid cannot be distilled by itself under the ordinary atmospheric pressure without undergoing decomposition. The colourless liquid, when gently heated, begins to decompose at  $75^{\circ}\text{C}$ ., the temperature gradually rising to  $92^{\circ}$ , at which point it remains constant for some time, gives off dense white fumes, and quickly turns to a dark blackish-brown colour, large quantities of a yellow gas having the peculiar sweetish smell of chloric oxide being evolved. After some time drops of a dark-coloured liquid, resembling bromine in appearance, distil over, the decomposition continuing in the retort; this dark liquid contained 94.77 per cent. of  $\text{ClO}_3\text{H}$ . On continuing the distillation

the decomposition became more rapid, and a sharp explosion occurred; the receiver was broken to fragments, whilst the residual liquid in the retort became colourless, and, on cooling, solidified to a crystalline mass which contained 87·76 per cent. of  $\text{ClO}_4\text{H}$ . I propose to investigate the composition and mode of formation of this singular body at a future time; it may possibly prove to be the black explosive substance described by Millon\* as produced by the action of organic matter on the so-called chloroperchloric acid. Perchloric acid likewise undergoes spontaneous decomposition at the ordinary atmospheric temperature, even when not exposed to light. The liquid, which when freshly prepared has a slight yellow colour, changes to a deep brown colour when kept in the dark for a few days; and after the lapse of about a fortnight the bulbs containing the acid are found to have exploded. Hence it is impossible to preserve pure perchloric acid for any length of time; it must be prepared when required from the crystallized acid.

## 2. *Perchloric Monohydrate*, $\text{ClO}_4\text{H} + \text{H}_2\text{O}$ .

The first hydrate of perchloric acid is the solid crystalline substance discovered by Serullas, and supposed by some chemists to be the anhydride†.

It is best prepared in the pure state by carefully adding water to perchloric acid; great heat is thereby evolved, and the crystals which form on cooling generally possess a slight yellow colour, which rapidly disappears when they are exposed to sunlight.

By partially melting the crystals several times, and separating the liquefied portions, the residue was found to be in a fit state for analysis, as is seen by the close approximation of the experimental numbers to the calculated composition in the following determinations.

*Analysis No. 1.*—Weight of colourless twice recrystallized acid employed 0·6710 grm. Treated according to the method already described, this quantity of crystallized acid yielded 0·7820 grm. of perchlorate of potassium, corresponding to 0·6698 grm.  $\text{ClO}_4\text{H} + \text{H}_2\text{O}$ , or to 84·55 per cent. of  $\text{ClO}_4\text{H}$ . Of this potassium-salt 0·6360 grm. lost by heating 0·2935 grm., and required 0·4925 grm. of silver for complete precipitation.

\* Ann. de Chim. et de Phys. sér. 3. t. vii. p. 333.

† Pelouze et Fremy, *Traité de Chimie*, "Acide Perchlorique," t. I. p. 418.

*Analysis No. 2.*—Another portion of crystallized acid, prepared and purified as above, was analysed with barium; 1·2345 grm. of acid yielded, according to the second method already described, 1·2155 grm. of sulphate of barium, corresponding to 1·2363 grm. of the monohydrate, or to 84·89 per cent. of  $\text{ClO}_4\text{H} + \text{H}_2\text{O}$ .

The following gives the results of the analyses:—

(a) *Composition of Crystallized or Monohydrated Perchloric Acid.*

		Calculated.	Found.	
			I.	II.
ClO <sub>4</sub> H	100·5	84·80	84·55	84·89
H <sub>2</sub> O	18·0	15·20		
	<u>118·5</u>	<u>100·00</u>		

(b) *Composition of the Potassium-Salt obtained from the crystallized acid*

		Calculated.	Found.
Cl	35·5	25·62	25·45
$\text{O}_4$	64·0	46·17	46·15
K	39·0	28·21	
	<u>138·5</u>	<u>100·00</u>	

The monohydrate of perchloric acid solidifies at the ordinary temperature to long silky needle-shaped crystals, which fume strongly in the air, most rapidly absorbing moisture, and immediately deliquescent on exposure. Placed in a thin bulb in a water-bath, the liquefied acid was found to freeze between  $49^{\circ}\cdot 5$  and  $50^{\circ}\cdot 0$  C., whilst the crystals melted between  $50^{\circ}\cdot 0$  and  $51^{\circ}\cdot 0$  C.; hence the melting- and freezing-points of this substance lie within a few tenths of  $50^{\circ}$  C. The specific gravity of the liquid monohydrate at  $50^{\circ}$  C. is 1·811; but the solid acid is considerably heavier, owing to the large contraction which the liquid undergoes on freezing. As has been described, the monohydrate splits up on heating into real perchloric acid and a less volatile aqueous acid; it therefore possesses no definite boiling-point. Heated to  $110^{\circ}$  C., the melted crystals enter into rapid decomposition, which, when once commenced, continues for some time, even when the thermometer sinks to  $95^{\circ}$ ; on further application of heat, the liquid becomes of a dark yellowish-brown colour, ebullition continues, and the temperature gradually

risks to  $203^{\circ}\text{C.}$ , at which point the mercury remains stationary, the colourless oily acid passing over unchanged.

Although not so violent in its action on organic matter as the pure acid, the fused monohydrate, when brought into contact with wood or paper, induces immediate combustion; and when dropped into water it combines with a hissing noise, forming the aqueous acid.

### 3. *Aqueous Perchloric Acid.*

There is certainly no other known acid in which the acid and the hydrates differ so widely from each other in their properties as is the case with perchloric acid,—the acid itself being a strongly fuming, volatile liquid, acting in a most violent manner on organic substances, and closely resembling nitric acid, whilst the monohydrate is a solid crystalline body, and the higher hydrates oily liquids having high boiling-points, being devoid of smell, and in outward appearance bearing a striking analogy to oil of vitriol.

When pure aqueous perchloric acid is concentrated by evaporation until white fumes are evolved, an acid is obtained on distillation which boils at a constant temperature (about  $203^{\circ}\text{C.}$ ), and has a constant composition. The strength of such a residual acid was determined by volumetric analysis with a standard solution of soda; it was found to contain 71.6 per cent.  $\text{ClO}_4\text{H}$ . As a control analysis, 0.6550 grm. of the same acid was neutralized by carbonate of barium, and yielded 0.5435 grm. of sulphate of barium, corresponding to 71.57 per cent.  $\text{H ClO}_4$ . Of this acid 25 cubic centims. was placed in a retort and distilled until three-fourths of the liquid had passed over; analysis with soda and with barium showed that it contained 72.18 per cent.  $\text{H ClO}_4$ .

The residual acid boiling at  $203^{\circ}\text{C.}$  left in the retort after distilling the solid monohydrate was found to possess a composition identical with the foregoing. Thus 1.1385 grm. of such residual acid yielded 1.136 grm. of perchlorate of potassium, corresponding to 72.4 per cent.  $\text{H ClO}_4$ ; and 0.954 grm. of the same acid furnished 0.7995 grm. of sulphate of barium, or contained 72.28 per cent.  $\text{H ClO}_4$ . It is thus evident that an acid containing less  $\text{H ClO}_4$  than 72.3 per cent. loses water, whilst an acid stronger than this loses real acid on distillation; so that both liquids yield a residual acid possessing a constant composition of 72.3 per cent.  $\text{H ClO}_4$ , and

boiling at  $203^{\circ}$  C. This per-centage corresponds, however, to no definite hydrate of simple atomic composition; an acid having the formula  $\text{H ClO}_4 + 2(\text{H}_2\text{O})$  containing 73.63 per cent.  $\text{H ClO}_4$ . Aqueous perchloric acid follows, therefore, the same general relations respecting composition and boiling-point which I have shown hold good for so many aqueous acids\*; namely, that the phenomena of constant composition and fixed boiling-point are in the above cases dependent mainly upon physical rather than upon chemical attractions. The specific gravity was found to be 1.709 at  $11^{\circ}$  C.

The reactions of aqueous perchloric acid have been sufficiently studied by other observers.

To the foregoing description of the properties of perchloric acid and its hydrates, I have to add the following analyses of a few of the salts of this acid which have hitherto not been examined. These analyses serve to confirm the fact that the estimation of perchloric acid by precipitation as potassium-salt in alcoholic solution is in accuracy second to few of the most exact methods of quantitative determination.

(1) *Perchlorate of Ammonium*,  $\text{NH}_4\text{ClO}_4$ .

This compound is one of the very few non-deliquescent salts of perchloric acid. It is anhydrous and isomorphous, as Mitscherlich has shown with the potassium-salt. The ammonium was estimated as the double potassium-salt, and the perchloric acid as potassium-salt insoluble in absolute alcohol. Analysis gave:—

	Calculated.	Found.
$\text{NH}_4$ . . . . . 18.0	15.32	15.43
$\text{ClO}_4$ . . . . . 99.5	84.68	84.63
117.5	100.00	100.06

(2) *Ammonio-perchlorate of Copper*,  $2(\text{NH}_4)\text{O Cu ClO}_4$ .

This salt is anhydrous, and was obtained in dark-blue crystals, which on exposure to air turn green, but do not deliquesce. It is best prepared by dissolving carbonate of copper in dilute perchloric acid, and adding an excess of ammonia. The crystals are readily obtained by pouring a layer of strong alcohol on to the surface of the

\* Roscoe, Chem. Soc. Quart. Journ. vol. xiii. p. 146.



aqueous solution. The salt is decomposed on boiling the solution, half the ammonia being liberated, and oxide of copper precipitated : this decomposition serves as a mode of determining the constitution of the salt, which must be considered to be  $\text{NH}_4\text{ClO}_4 + \text{NH}_4\text{CuO}$ . The total quantity of ammonia contained in the salt was likewise directly determined ; the perchloric acid being, as before, weighed as potassium-salt.

		Found.		
		I.	II.	III.
Cu	31·7	17·00	....	17·17
$2(\text{NH}_4)\text{O}$	52·0	27·98	28·30	
$\text{ClO}_4$	99·5	54·73	....	54·21
	<u>183·2</u>	<u>99·71</u>		
	Calculated.			
	17·30			
	28·38			
	54·32			
	<u>100·00</u>			

### (3) *Ferrous Perchlorate*, $\text{FeClO}_4 + 3\text{H}_2\text{O}$ .

This salt is obtained by dissolving metallic iron in aqueous perchloric acid ; it is highly deliquescent, it does not lose water at  $100^\circ\text{C}$ ., and when heated above that temperature it is decomposed. The small greenish-white crystals obtained by concentrating the solution were dried *in vacuo* over sulphuric acid. Analysis gave :—

		Found.		
		I.	II.	III.
Fe	28·0	15·41	15·75	15·53
$\text{ClO}_4$	99·5	54·55	55·47	54·56
$3\text{H}_2\text{O}$	54·0			
	<u>181·5</u>			
	Calculated.			
	15·44			
	54·80			
	29·76			
	<u>100·00</u>			

### (4) *Mercurous Perchlorate*, $\text{Hg}_2\text{ClO}_4 + 3\text{H}_2\text{O}$ .

Mercurous perchlorate is a highly deliquescent salt, obtained by dissolving black oxide of mercury in dilute perchloric acid. It does not lose water either at  $100^\circ\text{C}$ ., or *in vacuo* over sulphuric acid.

		Found.	
		I.	II.
$\text{Hg}_2$	200·0	56·57	56·63
$\text{ClO}_4$	99·5		
$3\text{H}_2\text{O}$	54·0		
	<u>353·5</u>		
	Calculated.		
	56·58		
	28·25		
	15·27		
	<u>100·00</u>		

(5) *Perchlorate of Lead*,  $2(\text{Pb ClO}_4) + 3\text{H}_2\text{O}$ .

Perchlorate of lead is an extremely deliquescent salt; it is best prepared by dissolving carbonate of lead in perchloric acid. The salt may be dried *in vacuo* over sulphuric acid without losing water.

		Found.	
		I.	II.
2Pb	207.0	44.82	44.74
2ClO <sub>4</sub>	199.0	42.22	
3H <sub>2</sub> O	54.0		
	<u>460.0</u>		
	100.00		

I desire to acknowledge the able assistance I have received from M. Schorlemmer in carrying out the above experiments.

December 19, 1861.

In consequence of the lamented Death of His Royal Highness The Prince Consort, no Meeting took place.

January 9, 1862.

Major-General SABINE, President, in the Chair.

The President, on the part of the Council, submitted to the Meeting the following Address of Condolence to be presented to the Queen:—

We, YOUR MAJESTY'S most dutiful and loyal subjects, the President, Council, and Fellows of the Royal Society of London for improving Natural Knowledge, desire humbly to offer to YOUR MAJESTY the sincere expression of our sorrow for the irreparable loss which YOUR MAJESTY and the nation have sustained by the death of HIS ROYAL HIGHNESS THE PRINCE CONSORT.

While we deeply share in the universal lamentation which this great calamity has called forth, we claim the mournful privilege of giving expression to our special grief for the loss of a Prince who, to his other high qualities and claims to the nation's esteem and gratitude, united a just appreciation of the importance to mankind

of those pursuits to which the Royal Society is devoted, and an earnest desire to aid in their advancement.

That it may please Divine Providence to soften the affliction of YOUR MAJESTY and Your Royal Family, and support you in this heavy trial, is the earnest wish and prayer of YOUR MAJESTY'S loyal and devoted subjects, the President, Council, and Fellows of the Royal Society of London.

On the motion of the Lord Chief Baron, seconded by Mr. Heywood, it was Resolved,—That the Fellows do most cordially concur in the Address now read from the Chair.

The Address was then signed by the President on behalf of the Council and Fellows.

The following communications were read :—

I. "Preliminary Note on the Nature and Qualities of Voltaic Currents." By GEORGE GORE, Esq. Communicated by Professor STOKES, Sec.R.S. Received January 9, 1862.

1. In a recent paper "On the production of Vibrations and Sounds by Electrolysis," I have shown that if a voltaic current of suitable quantity from two Grove's or five Smee's elements is passed by a mercury anode through a solution composed of 10 grains of cyanide of mercury and 100 grains of hydrate of potash, dissolved in  $2\frac{1}{4}$  ounces of hydrocyanic acid containing 5 per cent. of anhydrous acid, into an annular cathode of mercury about 2 or 3 inches diameter and  $\frac{1}{8}$ th of an inch wide, *visible* and symmetrical *vibrations* of the negative mercury, accompanied by definite *sounds*, are produced; and the current becomes *intermittent* as long as the vibrations continue, similar to a current made intermittent by means of an ordinary break-hammer.

2. If two voltaic currents of suitable and *equal quantity* (measured by a voltameter in the circuit), the one being generated by about eight Smee's elements of large immersed surface, and the other generated by twenty Smee's elements of small surface, are passed separately through the arrangement just described, vibrations and sounds are in each case produced; but with the current from the few

elements of large surface the amplitude of the vibrations is small and the sound high, whilst with the current from the many elements of small surface the amplitude of the vibrations is large and the sound base. These differences in the vibrations and sounds are still more conspicuous if a galvanometer of small resistance (*i. e.* with a short and thick wire) is substituted for the voltameter, and about four Smee's elements employed instead of the eight. In each of these experiments the voltameter (or galvanometer) is in the circuit with the cyanide solution; the quantities of the two currents are made equal by suitably adjusting the relative depths of immersion of the plates of the two batteries; and each experiment (with the voltameter) occupies 3 minutes. The size of the mercury electrodes has also been previously adjusted to the power of the current, so as to give continuous definite vibrations and sounds.

3. Further:—If a current from two Grove's or five Smee's elements of large surface is passed through a primary coil of about 250 feet of thick copper wire, through the cyanide solution and small-resistance galvanometer, the vibrations are moderate in size and the pitch of the sound is moderately high; but if the axis of the coil contains a massive bundle of soft iron wires, the vibrations are much larger and the pitch of the sound is much more base; and if the primary coil is surrounded by a secondary coil containing about 4000 feet of fine copper wire, the ends of which are closely united together, and the iron core is absent, the vibrations are very much smaller and the sound is much higher. In each case the *quantity* of the current, however, remains the same. If a voltameter is used instead of the galvanometer, a greater number of elements (about eight Smee's) must be employed, and the difference in the effects is then less striking. If a battery of much greater intensity, say twenty Smee's elements, is employed, no difference in the vibrations or sounds is produced by the introduction of the soft iron core, nor by closing the secondary coil.

*Do not voltaic currents therefore of equal quantities\* from different sources, or under different external conditions, like heat and light from different sources, possess different qualities?*

4. From these results (as well as from additional ones that I have

\* I employ the word "quantity" in its ordinary sense, *viz.* as that indicated by measurement of gases from decomposition of water in a voltameter.

obtained) it appears to me that voltaic electricity, like heat or light, may be viewed as consisting of *vibrations* or *successive impulses*, which under ordinary circumstances occupy so minute a period of time as to be inappreciable, but when acting under suitable conditions upon suitable substances, such as the metal and liquid referred to (1), the vibrations of the current are taken up by the substances, and the oscillations of the substances thereby produced are gradually increased by the synchronous impulses of the current until they become visible and attain their maximum (see paper "On the production of Vibrations and Sounds by Electrolysis," paragraph 11), like visible oscillations of a pendulum produced by minute synchronous mechanical impulses. This I beg leave to state as an hypothesis for the purpose of making the subject more clear and aiding future inquiry.

*Note by the Communicator.*

[The results mentioned in this paper are well worthy of attentive consideration, in relation to that curious and still mysterious phenomenon which the author is investigating with so much care. As regards, however, the conjecture thrown out by the author,—while the importance of such a conclusion as that of the existence of qualitative differences in permanent electric currents, according as few or many voltaic elements are concerned in their formation, or of periodicity as a *necessary* condition of a voltaic current, if fully established, cannot be overrated, the conclusion does not seem to the Communicator of the paper to be fairly deducible from the experiments described. It would rather seem that, from some cause yet to be investigated, the motion of the mercurial cathode, or rather the change of figure resulting from the motion, alters the total electromotive force or resistance (more probably the resistance) in the circuit, and thus, by altering the current, reacts upon the forces whereby the motion of the cathode is produced. In a circuit of small resistance, it might be expected according to this view that a smaller motion of the cathode would suffice to bring about a given change in the current, and a corresponding change in the force producing the motion, and accordingly that the period of the changes would be shorter than in a circuit of greater resistance, although the

*mean* currents in the two circuits, as measured by a galvanometer or voltameter, might be the same.]

## II. "On the Diurnal Tides of Port Leopold, North Somerset."

By the Rev. SAMUEL HAUGHTON, M.A., F.R.S., Fellow of Trinity College, Dublin. Received November 7, 1861.

### (Abstract.)

The present is the first of a series of communications on the tides of the Arctic Seas which the author hopes to lay before the Royal Society. The MS. materials at his disposal embrace both the Atlantic and Pacific Arctic Tides, for which he was indebted to the Hydrographer, Captain Washington, R.N., to Captain Collinson, R.N., Captain Sir F. Leopold M'Clintock, R.N., and Captain Rochfort Maguire, R.N.

The present paper discusses fully the diurnal tide of Port Leopold, which is most remarkable from the proportion which it bears to the semidiurnal tide, a proportion which is unusually large. From the discussion of this tide, the author is enabled to announce with confidence several results or laws which he had previously obtained and published from the discussion of the small diurnal tides of the coasts of Ireland.

These results are given in detail in the paper itself. In the concluding portion of the paper, the author calculates, from received dynamical theories, the depth of the Atlantic Canal, from the proportion of the Solar to the Lunar coefficient, from the Diurnal Solitidal and Lunitidal Intervals, and from the Age and Acceleration of the Luni-diurnal Tide.

He hopes to forward shortly the discussion of the Semidiurnal and Parallactic Tides of the same locality.

- III. "On the Posterior Lobes of the Cerebrum of the *Quadrumanus*." By WILLIAM HENRY FLOWER, Esq., F.R.C.S., Conservator of the Hunterian Museum, Royal College of Surgeons. Communicated by Dr. SHARPEY, Sec. R.S. Received November 20, 1861.

(Abstract.)

The substance of this paper is contained in one presented to the Society June 20th, 1861 (see Abstract in 'Proceedings,' vol. xi. p. 376), with which further observations since made have been incorporated. A more detailed description of the posterior lobes of the brain of *Cercopithecus*, *Macacus*, and *Cebus* is given, as well as an account of the same parts in *Presbytes* and *Hapale*. It is shown that the brain of the last-named and that of Man, placed at the opposite ends of an extensive series, present in the posterior lobes certain well-marked common characters, but that in the Marmoset this portion of the brain is proportionally more elongated, the calcarine fissure is more deeply cut, the hippocampus minor more prominent, and the posterior cornu patent to a greater extent.

The author having had an opportunity of dissecting the brain of a Lemur in a recent condition, has substituted a description of the cerebral characters of this animal for that of the Galago previously given, which having been long preserved in spirit, was not so well adapted for the purpose. In possessing a well-marked Sylvian fissure, a median lobe, a calcarine sulcus, and in the general character of the convolutions, the brains of members of this family are evidently formed upon the type common to the brain of Man and the higher families of *Quadrumanus*; but while the gradations of this type are tolerably regular and unbroken between *Homo* and *Hapale*, the Lemurs do not follow in the same line of degradation, and should rather be placed as a small subseries parallel to the lower part of the large series, but separated from it by the shortness of the posterior lobes, large size of the olfactory bulbs, and inferior characters of the cerebellum.

A Table is added, showing the comparative length of the posterior lobes in certain *Quadrumanus* and other *Mammalia*, measured upon a plan described in the paper.

- IV. "On the General Forms of the Symmetrical Properties of Plane Triangles." By THOMAS DOBSON, Esq., B.A., Head Master of the School-Frigate 'Conway,' Liverpool. Communicated by Capt. E. A. INGLEFIELD. Received December 3, 1861.

(Abstract.)

The symmetrical properties of plane triangles have been fully expounded in a series of six papers dated 1835, 1836, 1842, 1843, 1845, and 1848 respectively, in the 'Lady's and Gentleman's Diary,' published annually by the Stationers' Company of London. Either from being the work of different hands, or from the earlier papers having been written before the importance of symmetry in mathematical formulæ was duly appreciated, the series of papers leaves much to be desired as to uniformity of method.

By assuming the usual expressions for the area of a triangle, as in the present paper, with a few other expressions of an equally elementary nature, all the well-known symmetrical properties of plane triangles may be readily deduced, by a little skill in the combination of algebraical symbols, without leaving the plane of the triangle.

But the author has preferred to use a method which is at once general, simple, and uniform. This consists in referring the symmetrical points connected with a triangle to an indefinite plane, and establishing by an elementary process certain general formulæ, each of which can be made to yield several cognate plane properties when different definite positions are assigned to the plane of reference.

- V. "Note on Ethylene-Dichloride of Platinum." By P. GRIESS, Esq., and C. A. MARTINS, Ph.D. Communicated by Dr. HOFMANN. Received December 16, 1861.

About thirty years ago, Zeise obtained, by the action of dichloride of platinum upon alcohol, a compound which he found to have the following composition,



This formula was corroborated by the analysis of a series of compounds which this body forms with the chlorides of some of the



metals. The chloride-of-potassium compound, according to Zeise's researches, contains



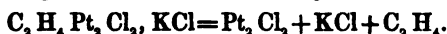
The chloride-of-ammonium compound has an analogous composition. Zeise further observed that his platinum compound unites directly with ammonia, producing a substance of the formula



The correctness of these formulæ Liebig, relying on certain theoretical conceptions, has called in question. The discussion which followed his remarks has, however, in no way decided the constitution of these compounds.

We have undertaken to prepare and analyse some new double compounds of this series, in order if possible to elucidate the constitution of Zeise's bodies.

We first endeavoured to establish the nature of the gas which under various conditions is evolved from Zeise's compounds. For this purpose we exposed the potassium-salts above mentioned to a temperature of  $200^\circ$ , and collected the gas which was evolved, over bromine water. In this manner an oily liquid was obtained, which was easily proved to be identical with dibromide of ethylene. The formation of the gas seems to ensue according to the following equation,



The formation of ethylene-gas, as well as the analysis of several salts which we prepared, seems to indicate that the original formulæ given by Zeise are correct, and that the existence of the group  $(\text{C}_2\text{H}_4)_2\text{O}$  assumed by Liebig in these substances is not supported by experimental evidence.

We have succeeded in combining ethylene-dichloride of platinum (this is the name which we propose for Zeise's compounds) with mono- and diatomic bases. We have also combined this substance with the chlorides of the bases.

The bodies thus obtained may be arranged in two series, comparable in many respects with certain classes of compounds which protochloride of platinum forms with organic bases. If ethylene-dichloride of platinum be viewed as the chloride of a monoatomic radical thus,

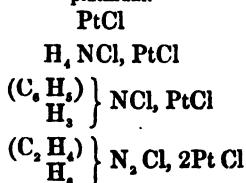


a very simple relation between the derivatives of this substance and

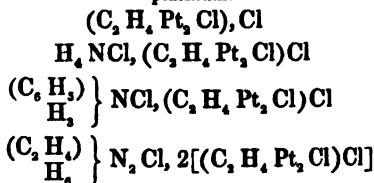
some of the compounds of protochloride of platinum becomes perceptible.

*First Series.*

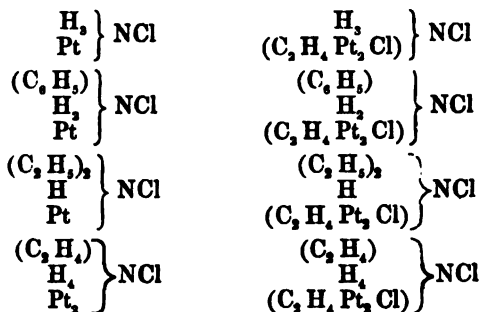
Compounds of protochloride of platinum.



Compounds of ethylene-dichloride of platinum.

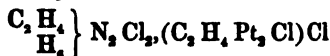


*Second Series.*

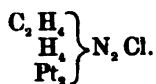


The compounds of ethylene-dichloride of platinum with ammonia and chloride of ammonium here mentioned have already been described by Zeise; the remainder, as well as the greater number of the protochloride-of-platinum bodies, are new.

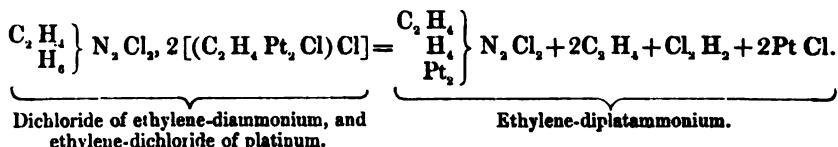
The detailed examination of these compounds, which are for the most part beautifully crystallized, is not yet completed; but we take this opportunity of mentioning an observation which seems to give a more decided support to the view we have expressed regarding the relation of the series of ethylene-dichloride-of-platinum and that of the protochloride-of-platinum compounds. If the aqueous solution of the easily soluble body



be boiled, a considerable quantity of gas is immediately cooled, and at the same time beautiful yellow, difficultly soluble needles are deposited containing



This reaction may be explained by the following equation :—



In conclusion it deserves to be noticed that the compounds of acetylene with subchloride of copper and other salts, which have been observed by Boettcher, Berthelot and others, may probably be classed with the group of the ethylene-dichloride of platinum. It is with the intention of testing this view that we are now engaged in an investigation of the deportment of protochloride of platinum with olefiant gas.

The observations described in this Note were made in Professor Hofmann's laboratory.

*January 16, 1862.*

**Dr. WILLIAM ALLEN MILLER**, Treasurer and Vice-President, in the Chair.

The following communications were read :—

- I. "On the Development of Striped Muscular Fibre in Man, Mammalia, and Birds." By **J. LOCKHART CLARKE**, Esq., F.R.S. Received November 21, 1861.

(Abstract.)

In the domestic fowl, until the beginning of the fifth day of incubation, the so-called voluntary muscular tissue consists only of a crowded multitude of free nuclei imbedded in a finely granular blastema; the nuclei are round, oval, pyriform, and somewhat angular, with granular contents. On the fifth and sixth days of incubation, fibres become superadded under two forms,—1st, as processes extending from the ends, or from the sides of nuclei; 2nd, as narrow bands, either uniformly delicate and pale, or bordered by darker outlines, and containing nuclei at variable intervals. They are most numerous near the surface of the layer, and probably belong, at least partly, to the muscular layer of the skin. In every case their first stage of development is conducted on one general plan, which consists in the fibrillation of the blastema along the sides of nuclei, to which the fibrillæ so formed become adherent. Sometimes these fibrillæ or lateral fibres enclose a single nucleus with conical processes of blastema, so that the object occasionally presents some resemblance to a fusiform nucleated cell. More frequently, however, they enclose a linear series of nuclei at variable distances from each other, but cemented together by blastema, which sometimes assumes around each a more or less definite shape. In the formation of the paler fibres, however, a series of neighbouring nuclei may sometimes be seen first to collect round themselves granular masses of a more or less fusiform appearance, and then to coalesce with each other, in an oblique or alternately imbricate way. Sometimes a series of the nuclei themselves overlies each other in an imbricate form like a

number of coins, and are cemented together by a common layer of blastema.

In the early part of the seventh day of incubation, numerous fibres of a much larger and more striking description suddenly make their appearance in the nucleated blastema. They originate, however, on the same general plan as the others, in a fibrillation of the blastema between, or along the sides of, a variable number of nuclei; but the process goes on to form aggregate masses of a much larger kind, and of a more or less oval, fusiform, or cylindrical shape, in which the nuclei are ultimately enclosed. Some of these bodies have a very striking resemblance to organic muscular-fibre-cells, which, according to my own opportunities of observation, are developed on the same general plan, that is, by the formation of sarcous substance, first, in the shape of fibres or lateral bands along the sides of a nucleus more or less encrusted with blastema, so that the *organic* muscular-fibre-cell would appear to represent an early stage in the development of the *striped* muscular fibre.

As incubation advances, the fibres acquire a tubular investment of the contractile or sarcous substance, which gradually increases in thickness or depth, and appears on each side as a band of corresponding breadth. As they grow in length, they also contract in diameter, and become of uniform structure throughout; while their nuclei rise nearer to the surface, and assume a more oval form. At this period the marks of striation, either longitudinal or transverse, are only faint and occasional.

By the fourteenth day of incubation, the entire substance of the fibres separates into longitudinal fibrillæ, which in turn become shortly resolved into particles or sarcous elements. After this the fibres continue to grow in thickness by the addition, to their surfaces, of new fibrillæ, which, as usual, are formed around nuclei encrusted with blastema cementing them, in such cases, to the original fibre.

In mammalia, although there are some particular but unimportant differences in the development of muscular fibre, the general plan is the same as in birds. The nuclei—at least in the ox, sheep, and pig—are larger, and have more distinct cell-walls or enveloping membranes. The fibres of the sheep or pig first make their appearance, in the foetus of from half to three-quarters of an inch in length, as thick and nearly parallel threads lying amongst a densely crowded

mass of free nuclei. When isolated, these fibres are seen to be attached to one or more of the nuclei by a variable quantity of blastema. Sometimes a single nucleus with conical processes of delicate granular substance is first enclosed by fine fibrillæ or lateral bands, which present somewhat the appearance of a *cell-wall*, so that the object has a certain resemblance to a nucleated fusiform cell with a fibre originating from one of its extremities. Sometimes several nuclei are cemented in a group around a fibre, and become subsequently covered by other fibres of the same kind; and sometimes they lie in linear series, either at some distance apart, or overlying each other to a certain extent like a series of coins. The lateral bands or fibres enclosing the nuclei extend around them as a tubular investment, which grows in thickness from without, but not always uniformly on all sides. In the process of longitudinal growth, the nuclei multiply by subdivision, become generally more oval, and approach nearer to the surface of the fibre, which at the same time contracts in diameter. The subsequent changes they pass through are nearly similar to those which occur in the chick.

In man the development of muscular fibre proceeds on the same general plan as in birds and mammalia, but differs from that of both in certain unimportant particulars. In the early stages there is no distinct appearance of those oval, cylindrical, and irregular masses observable in the chick on the seventh day of incubation and in the mammal at a corresponding period. In this respect there is a greater resemblance between the two latter classes than between man and either. In the human foetus, from about half to three-quarters of an inch in length, the first stage of development may be seen to commence by the formation of fine lateral bands or fibrillæ along one or both sides of one nucleus or more. When, however, there are more nuclei than one enclosed by the same lateral bands, they are always disposed in linear succession, with their longer axes in the direction of the fibre, and never occur in irregular groups, as is sometimes the case both in birds and mammals, in which, consequently, the same kind of fibres are often broader at first. Thus formed, they lie side by side in bundles of different sizes, to which new fibres or new fibrillæ are being continually added by a renewed process of development. Every fibre is the rudiment of several fibrillæ. At this period each lateral band constitutes a single fibrilla, which is often

resolved into sarcoous elements of great distinctness and beauty, while new and similar fibrillæ are developed along its sides in the way already explained. The subsequent series of changes do not differ materially from those that occur in the inferior classes.

It is evident that this description of the development of muscular fibre is entirely opposed to the cellular theory of Schwann; while it agrees in some points with that of Lebert (*Annales des Scien. Nat.* 1849-50), but more with that of Savory (*Phil. Trans.* 1855). In no instance have I found that nucleated *cells*, properly so called, are concerned in the office of development; for the finely granular blastema attached to the nuclei, although it frequently assumes the shape of a fusiform *cell*, is not invested with a *cell-wall*, in the proper sense of the word. Such an envelope, however, is sometimes simulated by the investing sarcoous substance or fine lateral fibrillæ when they are first laid down on the sides of the fusiform mass and meet each other at each extremity to form a single fibre or process. Indeed, according to my own observations, as already remarked, this is precisely the mode in which the *organic* muscular-fibre-cell is developed; so that the *striped* muscular fibre, instead of being the *product* of nucleated *cells*, would appear to be itself, at first, an instance or mode of cell-formation, which finds its prototype in the *organic* muscular fibre-cell, and in which the *cell-wall* is substituted and represented by the investing sarcoous substance.

## II. "On the Influence of Temperature on the Electric Conducting Power of the Metals." By A. MATTHIESSEN, Esq., F.R.S., and M. VON BOSE. Received December 5, 1861.

(Abstract.)

In the first part of the paper we have described the apparatus used for the experiments, together with the precautions taken to ensure correct results; in the second we have given the results obtained with the pure metals—silver, copper, gold, zinc, tin, arsenic, antimony, bismuth, mercury—and the metalloid tellurium. The conducting power of the wires, or bars of each, was determined at about 12°, 25°, 40°, 55°, 70°, 85°, and 100° C.; and from the mean of the eight observations made with each wire (four at each temperature on heating,

and four on cooling), we deduced a formula by the method of least squares for the correction of the conducting power for temperature. It was found that the conducting power or resistance of a metal does not decrease or increase in direct ratio to the temperature, as stated by Becquerel\*, Arndsten†, and Siemens‡, who assume that the formula for the correction of resistance for temperature between 0°–100° may be expressed by

$$\lambda = x + yt,$$

but that, on the contrary, the formula must be

$$\lambda = x + yt + \gamma t^2,$$

where  $\lambda$  is the resistance at  $t$  degrees,  $x$  the resistance at 0°, and  $y$  and  $\gamma$  constants. One fact seems to have escaped the observation of former experimenters, namely, that when a wire of a metal is heated for the first time to 100° and again cooled, an alteration in the conducting power takes place; with most metals it is necessary to heat them for several days before their conducting power becomes constant. In the third part we have deduced from the results obtained, the law that *all pure metals in a solid state vary in conducting power to the same extent between 0° and 100° C.* In cases where very great accuracy is required, it is absolutely necessary to experiment on the conductor itself; for we have found almost the same differences between formulæ obtained for wires of the same metal as between the mean of those deduced for the different metals. This behaviour may be attributed to the fact that the molecular arrangement is not the same even in wires of the same metal; for we find that copper wires, when kept at 100° for several days, behave very differently from each other: thus, in the case of the three copper wires experimented with, wire 1 increased in conducting power almost to the same extent as if it had been annealed, wire 2 partially so, and wire 3 hardly at all. With bismuth, wire 1 increased its conducting power 16 per cent.; wire 2, 19 per cent.; and wire 3, 12 per cent. Again, in the case of cadmium, which becomes quite brittle and crystalline at 80° (for cadmium may be powdered in a hot mortar), we found the formula for each wire very different. On the other hand, the formulæ of the wires of those metals which, after being kept at 100° for some time, show a very slight or no alteration in the conducting power

\* Ann. de Chim. et de Phys. (3) xvii. 242.

† Pogg. Ann. civ. 1.

‡ Pogg. Ann. cxiii. 91.



on again being cooled, agree very closely with each other. Compare those of lead, tin, and mercury.

*Metalloids conduct electricity better when heated than when cold.* Hittorf\* proved this to be the case with selenium. Gas-coke and graphite†, and the gases‡, follow the same law. Tellurium, when first heated to 70° or 80° C., behaves as a metal, that is to say, it loses in conducting power up to that temperature, when it then begins to gain. The temperature of the turning-point becomes lower after each day's heating, until, as with the first and third bars experimented with, it is below the lowest temperature at which observations were made. Taking the first observed conducting power of each bar = 100, we found that the conducting power of bar 1 had decreased after thirteen days' heating to 4, where it then remained constant; that of bar 2, after thirty-two days, became constant at 19; and that of bar 3, after thirty-three days, at 6. With bar 2 the conducting power decreased up to 29°·4, when it began again to increase. The behaviour of tellurium is therefore intermediate between that of the metals and that of the metalloids.

III. "Notes of Researches on the Poly-Ammonias."—No. XIX.  
Aromatic Diamines. By A. W. HOFMANN, LL.D., F.R.S.  
Received December 16, 1861.

Whilst engaged in the examination of the polyatomic ammonias of the ethylene-series, I have repeatedly endeavoured to produce the diatomic bases corresponding to the aromatic monamines. The composition and general characters of these compounds were sufficiently indicated by the examination of ethylene-diamine. The simple relation which the latter body bears to ethylamine,



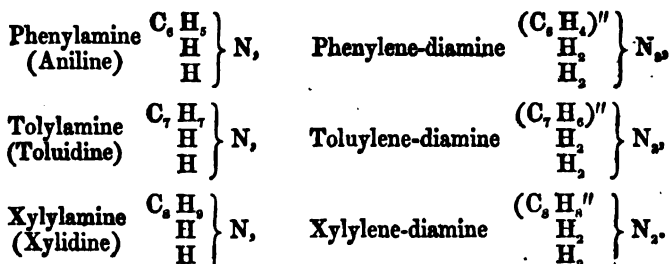
could leave no doubt regarding the existence of a series of diatomic aromatic ammonias similarly related to aniline and its homologues.

\* Pogg. Ann. lxxxvi. 214.

† Phil. Trans. 1858, p. 586.

‡ Ann. de Chim. et de Phys. (3) xxxix. 355.

The second column of the following Table sets forth the series of substances thus theoretically suggested :



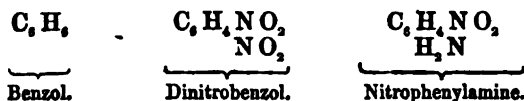
The method for producing these diatomic compounds appeared likewise obvious. Bearing in mind the simple transformation of benzol into nitrobenzol and aniline,



the idea very naturally suggested itself, to look to dinitrobenzol as the source from which phenylene-diamine might reasonably be expected to arise.



Nor have chemists failed to pursue the path pointed out by theory. In conjunction with Dr. Muspratt I have myself, many years ago, examined this reaction. We did not, however, succeed in producing the desired result, although our exertions were rewarded by the discovery of nitrophenylamine (nitraniline),



which, being the first basic nitro-compound with which chemists became acquainted, withdrew our attention for the time from the original object of the inquiry.

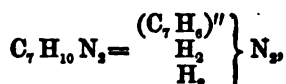
Nitrophenylamine being obviously the first product of the action of reducing agents on dinitrobenzol, it appeared very probable that the further reduction of the nitro-base or the prolonged treatment of dinitrobenzol itself might furnish the compound. I have repeatedly

tried to accomplish the final reduction of nitrophenylamine by the protracted action of sulphide of ammonium or potassium, without, however, obtaining definite results. Nor have Messrs. Church and Perkin, who have examined the action of nascent hydrogen upon dinitrobenzol, been more successful. Their experiments have led them to the discovery of an interesting new compound, nitroso-phenylene; but the formation of a diatomic ammonia was not observed. M. Zinin, on the other hand, to whom science is indebted for the important discovery of the reduction of nitro-compounds, has been more fortunate: by exhausting the action of sulphide of ammonium upon dinitrobenzol, this chemist has in fact obtained a substance to which he attributed the formula set forth for phenylene-diamine. The properties of the body described by Zinin under the name of semi-benzidam, are, however, far from those which might have been anticipated in the case of such a compound. Distillation of an alcoholic solution of dinitrobenzol with sulphide of ammonium, according to Zinin (Gerhardt, *Traité*, vol. iii. p. 104), leaves a brown resinous substance, soluble in water and containing much free sulphur. By dissolving it in boiling alcohol or ether, the compound is deposited, on cooling, in yellow flakes, melting under water to a brown resinous mass, which, on exposure to the atmosphere, rapidly assumes a green colour. This certainly is not the phenylene-diamine of our theoretical conceptions; and more than a year ago, when submitting to the Society some experiments on the action of nitrous acid upon nitrophenylene-diamine\*, I was led to express this opinion:—"Those chemists who have had an opportunity of becoming acquainted with the well-defined properties of ethylene-diamine, will not be easily persuaded to consider the uncouth dinitrobenzol-product—sometimes appearing in brown flakes, sometimes as a yellow resin, rapidly turning green in contact with the air—as standing to smooth phenylamine in a relation similar to that which obtains between ethylene-diamine and ethylamine." I have in no way to retract the opinion then expressed. The diatomic ammonia of the phenyl-series has obviously never been observed in a pure state. Phenylene-diamine and the homologous diatomic aromatic bases are as well-defined substances as their collateral monamines. This class of bodies is, in fact, characterized by an extraordinary crystallizing power,—both

\* Proceedings of the Royal Society, vol. x. p. 495.

the bases and their salts being easily obtained in crystals, which are often capable of measurement.

It was under peculiar circumstances that my attention was drawn again to the study of the aromatic diamines. I am indebted to Dr. Alphons Oppenheim for the communication of a specimen of a crystallized base, which had been obtained as a secondary product in the Aniline Works of M. Ch. Collin of Paris. The first combustions proved to me that this substance was one of the diatomic compounds which I had repeatedly endeavoured to produce. The crystals were found to contain



which is the formula of toluylene-diamine, the primary diamine of the toluyl-series. The intimate relation of this compound with the ethylene-bases, which I have lately studied, induced me to pursue the subject further. M. Ch. Collin has had the kindness to furnish me with a most liberal supply of this interesting substance, accompanied by a very lucid and elaborate statement of the circumstances under which it is produced, drawn up by Dr. Coblentz, the chemical director of the factory. I have thus been enabled to verify the formula above given by the analysis of several salts.

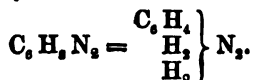
There could be no doubt about the reaction which, in the manufacturing processes of M. Collin, had given rise to the formation of this substance. It obviously owed its origin to dinitrotoluol accidentally produced from the toluol invariably present in commercial benzol. Experiments have not failed to verify this view. Dinitrotoluol, prepared by the usual process from toluol, when distilled with a mixture of iron and acetic acid—the method of reduction now generally adopted in the manufacture of aniline—has furnished the crystalline alkaloid of M. Collin with all its properties. The identity was proved moreover by analysis.

The rest is rapidly told. The examination was at once extended to the dinitro-compounds of the homologues of toluol, and more especially to dinitrobenzol. The behaviour of these substances under the influence of acetate of iron, as might have been expected, is perfectly analogous to that of dinitrotoluol. It is my intention to lay before the Society a detailed account of the diatomic bases which

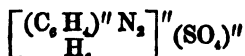
are thus produced. For the present I will mention only some of the properties of phenylene-diamine and toluylene-diamine, in order to give an idea of the general character of this class of bases.

*Phenylene-diamine.*

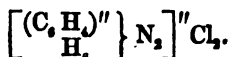
Freshly distilled, it presents itself as a slightly coloured heavy oil, which, like phenylamine, has a tendency to assume a brown coloration on exposure to the atmosphere. The base remains liquid often for days, and then gradually solidifies into a mass of crystals, which become hard and white by washing with ether. The fusing-point of phenylene-diamine is  $63^{\circ}$ . Its boiling-point is near  $280^{\circ}$ ; it distils without alteration. This substance is very soluble in water and alcohol; the solutions have a distinctly alkaline reaction. It is far less soluble in ether. Phenylene-diamine contains



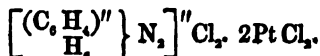
This base, as might have been expected, is diacid. A beautifully crystallized sulphate was found to contain



The dichloride is very soluble in water, but was easily crystallized from concentrated hydrochloric acid. It was found to contain—



Addition of dichloride of platinum to the solution of the chloride furnishes the platinum-salt, which crystallizes in splendid needles of the composition



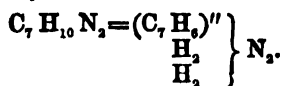
Phenylene-diamine is remarkable for the facility with which its salts, as well as its other derivatives, crystallize. In this respect it worthily emulates its monatomic correlative, phenylamine. The bromide and iodide are separated at once, in the form of crystalline masses, when phenylene-diamine is brought in contact with the respective acids. The salts thus produced crystallize splendidly from water, and more especially from alcohol. The nitrate and oxalate are not less beautiful. The salts of phenylene-diamine are readily decomposed by the fixed caustic alkalis; the base is thus separated

in oily globules, which only gradually solidify. Ammonia likewise separates the phenylene-diamine from its saline compounds; the slightest excess, however, redissolves it, and the solution is apt to become brown, and then contains products of transformation. This observation explains in a measure why the diatomic base cannot be conveniently obtained by the usual method of reduction by sulphide of ammonium.

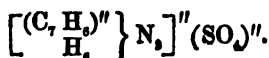
*Toluylene-diamine.*

This substance is a crystalline solid. It dissolves freely in water, forming an alkaline solution. It is likewise soluble in alcohol, and less so in ether. Toluylene-diamine is one of the most beautiful compounds I have ever seen. From boiling water it crystallizes in needles, which frequently acquire an inch in length. Like phenylene-diamine, this compound is apt to assume a yellowish tint in contact with the air. Crystallization from water does not remove this tint, which only yields to treatment with animal charcoal. The aqueous solution of toluylene-diamine rapidly acquires a dark-brown colour. The new substance fuses at  $99^{\circ}$ , and distils without change; the boiling-point is above  $280^{\circ}$ , a little higher than that of phenylene-diamine. I shall, however, determine the boiling-points more accurately as soon as I shall have procured myself larger quantities of both substances.

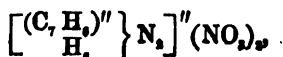
The analysis of toluylene-diamine has led to the expression



This formula was verified by the examination of a sulphate crystallizing in perfectly well-formed, long and rather thin prisms, apt to assume a beautiful pink colour, which were found to contain

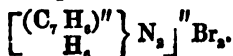


The nitrate forms long needles,

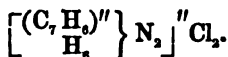


very soluble in water and alcohol.

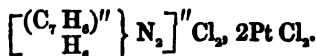
The bromide crystallizes in short prisms, likewise soluble in water and alcohol, having the composition



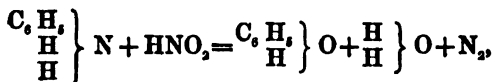
The chloride is more soluble and somewhat less easily crystallized from water, but it may, like the corresponding phenylene-diamine compound, be crystallized from hydrochloric acid. It contains



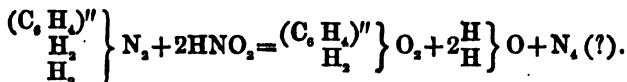
The platinum-salt crystallizes in golden scales. It is somewhat soluble in water, and is therefore conveniently washed with alcohol. The formula of this substance is



The substances of which I have submitted a short account to the Society are capable of furnishing an almost endless variety of derivatives. They are acted upon by cyanogen, chloride of cyanogen, by the chlorides of the acid radicals (chloride of acetyl and chloride of benzoyl), by the iodides of the alcohol-radicals, by disulphide of carbon, &c., forming a series of substances most of them remarkably well crystallized. Their composition being nearly always indicated in advance by theory, it is not my intention to examine these various derivatives in detail, but I shall avail myself of the two easily accessible diamines which I have described, for the purpose of establishing by a few numbers the chief characteristics of the diatomic bases corresponding to the aromatic monamines. I propose more especially to examine the deportment of these substances under the influence of nitrous acid. The action of nitrous acid upon aniline furnishing phenyl-alcohol,

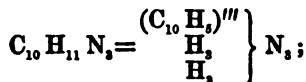


there is some hope of meeting, in the analogous decomposition of phenylene-diamine with the diatomic phenylene-alcohol (phenyl-glycol),

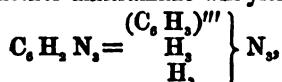


The facility with which acetate of iron effects the reduction of nitro-compounds in cases in which the sulphide of ammonium acts but slowly, or is altogether inadmissible on account of secondary decompositions which it may induce, suggests this method for the pro-

duction of the aromatic bases of higher atomidity, which are at present unknown. Trinitronaphtaline might thus yield a basic compound,



and even the triatomic base of the phenyl-series might possibly be obtained in this manner; for although we have not at present trinitrobenzol at our disposal, we could submit the nitro-bases themselves to further amidation. I have satisfied myself by experiment that phenylene-diamine may be just as well obtained by the reduction of nitraniline as of dinitrobenzol; and it deserves therefore to be ascertained whether dinitraniline will yield the compound



which would be the first aromatic triamine.

In conclusion, I may be permitted to express my best thanks to MM. Ch. Collin and Coblenz for the liberal manner in which they have furnished me the materials for the experiments described. By facilitating the scientific elaboration of the new diatomic compounds, these gentlemen have endeavoured most gracefully to acknowledge the debt of gratitude which the aniline-industry owes to theoretical inquiries in organic chemistry.

*January 23, 1862.*

Major-General SABINE, R.A., President, in the Chair.

The following communications were read :—

I. "Contributions towards the History of the Monamines."—

No. V. Action of Chloracetic Ether on Triethylamine and Triethylphosphine." By A. W. HOFMANN, LL.D., F.R.S.

Received December 16, 1861.

I am indebted to Mr. C. E. Groves for a considerable quantity of the ethyl-bases, which he has prepared by the action of ammonia upon iodide of ethyl, in order to test on a large scale the method of separating the three compounds by means of oxalic ether\*, which I

\* Proceedings of the Royal Society, vol. xi. p. 66.

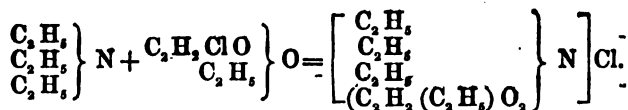


have lately proposed\*. This circumstance has enabled me to submit these substances, and more especially diethylamine and triethylamine, to a more thorough examination than they had hitherto received.

Reserving a detailed communication upon this subject to a future occasion, I beg leave to submit to the Royal Society a short account of some of the substances observed in the course of these experiments.

*Action of Chloracetic Ether upon Triethylamine.*

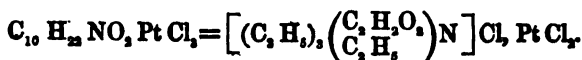
A mixture of triethylamine and chloracetic ether, both carefully dried, was exposed in a sealed glass tube for several hours to a temperature of 100°. On cooling, the mixture was found to have deposited some crystals (chloride of triethylammonium), and on opening the tube a small quantity of a gas, burning with a green-edged flame, escaped. Repeated experiments showed that these phenomena are due to secondary reactions. The principal product of the action of chloracetic ether upon triethylamine is the chloride of an ammonium containing in the place of the hydrogen three equivalents of ethyl and one equivalent of a complex atom consisting of the elements which in chloracetic ether are united with the chlorine,



The nature of the reaction was fixed by the analysis of the platinum-salt of this complex metal. Addition of dichloride of platinum to the aqueous solution of the product of the reaction produces a rather difficultly soluble crystalline precipitate, which by several crystallizations may be obtained in a state of perfect purity, the platinum-salt of triethylammonium, which is exceedingly soluble, remaining in the mother-liquor.

\* The separation of the ethyl-bases has been since repeatedly carried out. The process, as described in the 'Proceedings,' admits of a slight improvement. I proposed to separate the mixture of diethyloxamide and diethyloxamate of ethyl by filtration, and to purify the former by recrystallization from boiling water, the latter by exposure to a temperature of 0°. The separation is simpler and more perfect by submitting the mixture at once to the action of boiling water, when diethyloxamide dissolves, the diethyloxamate of ethyl remaining as an insoluble layer floating upon the hot solution, from which it may be separated by a tap-funnel.

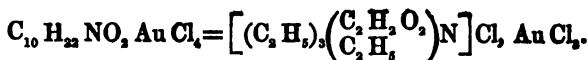
The new platinum-salt crystallizes in splendid, well-formed crystals of rhombic habitus. They were found to contain



The chloride corresponding to the platinum-salt is readily obtained by the action upon it of sulphuretted hydrogen. By evaporation *in vacuo* over sulphuric acid, it remains in the form of long needles, extremely soluble both in water and alcohol. From the latter solvent it may be recrystallized. The crystals, however, owing to their deliquescent character, are but little adapted for analysis. I have therefore been satisfied to corroborate the formula of the platinum-salt by the examination of the corresponding gold-compound.

The gold-salt crystallizes in needles, which, since they fuse at  $100^\circ$ , have to be dried *in vacuo*.

Formula:

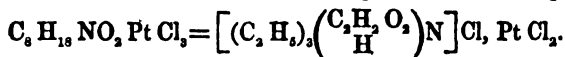


I have not been able to obtain the base corresponding to this series of salts. The chloride, when treated with oxide of silver, yields chloride of silver and a solution which, on evaporation, solidifies into a radio-crystalline compound. Although perfectly neutral to test-paper, this substance forms, with hydrochloric and hydriodic acids, well-defined salts which belong, however, to another series. The liquid obtained by the action of oxide of silver, in addition to the crystalline compound, contains alcohol which may be separated by fractional distillation. The crystalline substance formed under the above circumstances is rather deliquescent, and was therefore not submitted to analysis. To obtain some insight into its nature, the platinum- and gold-salts, as well as the beautifully crystallized nitrate and iodide, were examined. Their analysis has proved that these salts differ from the saline compound formed by the action of chloracetic ether upon triethylamine, by the substitution of hydrogen for an equivalent quantity of ethyl,—a difference of composition which might have been inferred from the elimination of alcohol in the process of transformation.

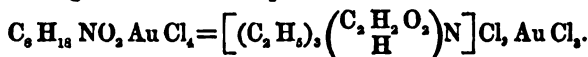
The new set of salts containing only three equivalents of ethyl, I may, for the sake of convenience, designate them as the triethylated

compounds in contradistinction to the former class, which are tetra-ethylated.

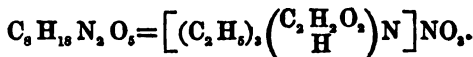
The platinum-salt is readily obtained by dissolving the triethylated base in hydrochloric acid, and adding dichloride of platinum. The precipitate may be crystallized from boiling water without decomposition. The salt forms beautiful rhombic prisms of the composition



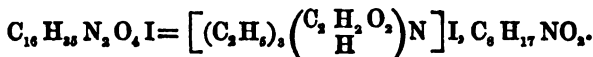
The gold-salt crystallizes in needles difficultly soluble in cold, easily soluble in boiling water, in which they fuse. Their composition is analogous to that of the platinum-salt,



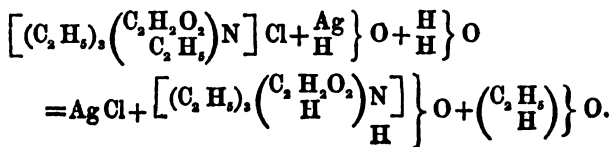
The *nitrate* is formed by dissolving the triethylated compound in nitric acid, evaporating the solution to dryness, dissolving the residue in alcohol, and adding ether, when the salt crystallizes out in splendid needles very soluble in water. The combustion of the compound led to the formula.



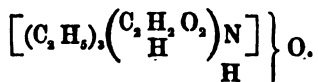
The only additional salt of this series which I have examined is the iodide. It is formed by dissolving the triethylated compound in hydriodic acid, evaporating, washing the crystalline residue with strong alcohol, and recrystallizing from boiling alcohol. The crystals are generally well-formed; they are extremely soluble in water. The composition of this salt presents some interest. Analysis proved it to contain



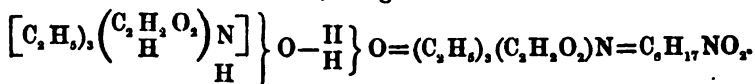
From the analysis of these salts it is evident that the action of oxide of silver upon the compound of triethylamine with chloroacetic ether is twofold: in the first place, the chloride is converted into the corresponding base; in the second place, this base loses an equivalent of ethyl, which separates in the form of alcohol:



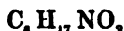
The crystalline substance which remains after treatment of the tetrethylated chloride with oxide of silver would thus be the mon-atomic base



There is, however, some reason to believe that the compound is decomposed in the moment of the formation, and that the crystals contain one molecule of water less, being in fact

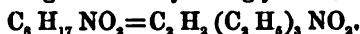


The crystalline product has no alkaline reaction whatever; moreover, we have seen the body

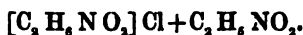


associated with the iodide in the compound above described.

I lay some stress upon these facts, since they would lend at once an additional interest to the compound under consideration, which would thus appear in the light of triethylated glyocoll,



It deserves to be noticed that normal glyocoll exhibits a tendency to form compounds similar in constitution to the iodide above described, one of the hydrochloric acid compounds being represented by the formula



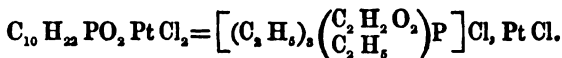
The new triethylated compound, whatever its constitution may be, is remarkable for its stability. Ebullition with the strongest potash is without effect upon it. I have boiled it with fuming nitric acid for hours without producing any alteration. A current of nitrous acid passed through the nitric acid solution leaves it unchanged. Evaporated to dryness, the residue gave, with hydrochloric acid and platinum- or gold-solution, the original platinum- and gold-salts.

When submitted to the action of heat, the triethylated compound is entirely decomposed. A powerful alkaline liquid distils, whilst a charred residue remains behind. The alkaline distillate contains a highly volatile base, forming with hydrochloric acid and dichloride of platinum a rather soluble salt. I infer from some preliminary platinum-determinations that the base thus obtained is by no means triethylamine. Further experiments are necessary to clear up the

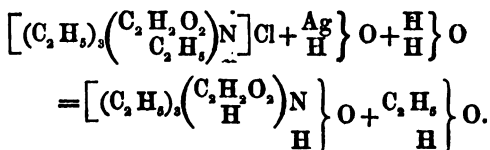
nature of this substance, the examination of which is likely to throw some light on the constitution of the compound whence it is derived.

*Action of Chloracetic Ether upon Triethylphosphine.*

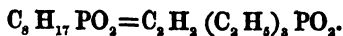
The reactions which I have described were also applied to triethylphosphine. Repetition of all the phenomena previously observed with triethylamine. Triethylphosphine and chloracetic ether combine with evolution of heat and formation of a brownish liquid of considerable consistency. If somewhat larger quantities are to be mixed, it is desirable to moderate the action by the presence of a volume of anhydrous ether, equal or greater than the aggregate bulk of the two liquids. Dissolved in water, separated by filtration or distillation from the excess of chloracetic ether employed, and mixed with dichloride of platinum, the new chloride furnishes a beautifully crystallized platinum-salt, which, after several crystallizations from boiling water, exhibits the composition



Submitted to the action of oxide of silver, the chloride contained in this platinum-salt undergoes the same change which was observed in the corresponding nitrogen compound,

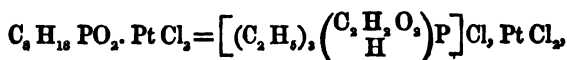


It is scarcely necessary to point out the perfect analogy of the new phosphoretted compounds with the corresponding bodies in the nitrogen-series. Whatever view be entertained of the latter, must also be taken regarding the former. Conceived in the anhydrous condition, the product obtained by the action of oxide of silver upon the chloride may be considered as phosphoretted glycoll with three equivalents of ethyl in the place of three of hydrogen,

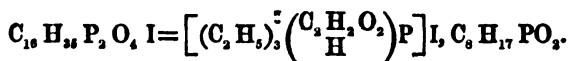


The phosphoretted compound resembles in its properties the substance derived from triethylamine. The aqueous solution, when evaporated *in vacuo*, solidifies into a radiated crystalline mass. I

have been satisfied to fix the composition of this body by the analysis of the well-crystallized platinum-salt, which was found to contain



and by that of the iodide. The latter was formed by precipitating the platinum-salt by sulphuretted hydrogen, decomposing the chloride formed in this manner by oxide of silver, and dissolving the triethylated compound in hydriodic acid. The solution was evaporated to dryness, the residue washed with absolute alcohol and recrystallized from the same liquid. This iodide is more soluble and less beautiful than the corresponding compound in the nitrogen-series. Analysis showed, however, that it has an analogous composition, viz.



Whatever view may be taken respecting the composition of the compounds described in the preceding pages, it is obvious that chloracetic ether, in its action on triethylamine and triethylphosphine, exhibits the deportment of one molecule of hydrochloric acid, and that the complex atom,



which in chloracetic ether is united with one equivalent of chlorine, represents in the compounds thus produced one equivalent of hydrogen. These substances are ammonium-salts of double substitution, the compound atom, which replaces one of the hydrogen equivalents of the ammonium, containing itself an equivalent of ethyl, substituted in this atom for the hydrogen originally present.

Compounds of a similar construction have been previously obtained. In his beautiful researches on the amidic acids, M. Cahours has proved that the ethers of benzamic, toluylic, and cumylic acids exhibit the same tendency to combine with acids which characterizes the amidic acids themselves. In these ethers the ethyl-atom may be exchanged at pleasure for hydrogen and metals; it obviously has been introduced into the molecular system of these bodies by what may be called a secondary substitution. The constitution of the compounds obtained from the first salts by the action of oxide of silver is less transparent. It may be that there is between these two classes a relation similar to that which obtains between amidic ethers

and amidic acids. But they may be, as I have pointed out, interpreted in another way. The question thus presented is accessible to experiment, being capable of solution in a variety of ways; and it appears useful to postpone further speculation upon this subject until it may be raised upon a broader experimental foundation.

II. "Additional Observations and Experiments on the Influence of Physical Agents in the Development of the Tadpole and the Frog." By JOHN HIGGINBOTTOM, Esq., F.R.S.  
Received Dec. 24, 1861.

In a former paper "On the Influence of Physical Agents on the development of the Tadpole of the Triton and the Frog," which the Royal Society honoured with a place in the Philosophical Transactions for 1850, experiments were detailed to prove that the ovum of the frog (the *Rana temporaria*) underwent its metamorphosis in the absence of light, contrary to the experiments of Dr. W. F. Edwards of Paris, related in his work 'On the Influence of Physical Agents on Life.'

My most satisfactory experiment was made in a rock cellar 30 feet deep, where no solar light ever entered; the mean temperature of the cellar was 51° Fahr.,—I believe, the lowest temperature at which the transformation could be effected.

The ova of the frog, just deposited, were placed in the cellar on the 11th of March, and on the 31st of October the first was fully developed in the form of a frog; while other ova deposited on the same day, which were placed in a shady part of a room at 60° Fahr. and covered with several folds of black calico, were fully developed on May 22nd, twenty-three weeks earlier than those in the cellar.

The experiment proved that the development of the frog depended upon the temperature, and not upon the presence of light. I found by other experiments that those in the light, and those deprived of light, were equally developed if placed at the same temperature. I observed that an excess of light retarded the development.

1. *On the Influence of Light on the Ovum.*

My former experiments not being parallel with those of Dr. Edwards, I was desirous of following his steps. Dr. Edwards says, in his work

above referred to, Part iv. chapter 15, 'On the Influence of Light upon the Development of the Body,' "This process, previous to birth, is generally carried on in the dark; there are, however, animals whose impregnated eggs are hatched, notwithstanding their exposure to the rays of the sun. Of this number are the Batrachians. I wished to determine what influence light independent of heat might exert upon this kind of development. With this view I placed some spawn of the frog in water in a vessel which was rendered impermeable to light by dark paper. The other vessel was transparent; they were exposed to the same degree of temperature, but the transparent vessel received the rays of the sun. The eggs exposed to the light were developed in succession; of those in the dark, none did well; in some, however, I remarked unequivocal indications of the transformation of the embryo."

Dr. Edwards does not mention the depth of water in the vessel in which he put the spawn of the frog, which he "rendered impermeable to light." If it were a few inches in depth, it would materially prevent the transformation of the embryo.

I commenced my experiment in a pool which had been the habitat of frogs (the *Rana temporaria*) for several years.

Experiment 1st.—I put a quantity of spawn, just deposited, into a box perforated with small holes, so as to admit a free current of water through it, and placed it about 3 feet below the surface of the water; all the ova perished.

The next experiments were made in an aquarium 20 inches deep, containing seventeen gallons of water at 60° Fahr.

Experiment 2nd.—A quantity of spawn was put into the water, which fell to the bottom of the aquarium; the spawn when first deposited by the frog, is specifically heavier than the water\*. The ova enlarged as usual, but did not arrive satisfactorily through the branchial state; most of the ova appeared to undergo no change whatever.

\* It is found that the frog usually deposits her spawn in shallow water, often close to the edge of a pool favourable to its development; at first it is very adhesive, and adheres firmly to the ground or weeds where it is deposited. The same condition may be observed when the animal deposits her spawn in an earthen-ware vessel. In this neighbourhood the deposition begins about the end of February or the first days of March, and continues fourteen or sixteen days, when the frogs quit the water.



**Experiment 3rd.**—Some spawn was placed 8 inches below the surface of the water ; but none of the ova passed through the branchial state.

**Experiment 4th.**—A quantity of spawn was placed on rock work near the surface of the water. Nearly all the ova passed satisfactorily through the branchial state to the formation of tadpoles ; each of the experiments was made at the same time and at the same temperature.

**Experiment 5th.**—A quantity of spawn was put into two round shallow dishes, each containing two pints of water, which were placed on the stand of the aquarium at the same time as in the former experiments ; nearly all did well ; and during the full branchial or fish-like state, great numbers of the embryos had placed themselves close to the margin of the water, forming a dark circle, with their branchiæ nearly exposed to the atmospheric air. They do not appear to feed during this period on the jelly—their first food ; atmospheric respiration seems more needful than food for their existence for several days during their full branchial state\*.

There are two distinct metamorphoses from the ovum to the full development of the frog: the first from the branchial or fish-like state to that of the tadpole ; the second from the tadpole to that of a frog, —the first requiring for its existence a close approximation to the atmospheric air, the second requiring full atmospheric respiration, to which I shall hereafter refer.

The branchial state continues about nine days, from the first budgings of the branchiæ to their absorption. About the seventh day the branchiæ are absorbed on the *right side*, indeed so quickly that I have observed that scores have lost them during one night, whilst the branchiæ on the left side have apparently been perfect ; but these in their turn become absorbed during the next day ; the respiration of this newly formed tadpole now depends on the internal gills and

\* The ova of the toad appear to be less tenacious of life than those of the frog. I have observed, when they are placed 4 or 5 inches below the surface of the water, they all die ; but they live when the long jelly lines in which the ova are enclosed are floated on weeds, or on a network of sticks on the surface of the water. In my note-book on the tadpole of the toad in 1849, I found that after the gelatinous lines began to break up on the surface of the water, and the ova to separate from them, a number of the eggs undergoing the metamorphosis adhered to the sticks and weed, but those falling to the bottom of the vessel perished.

cutaneous surface. The gill-opening for the passage of water is very apparent on the left side, but there is none on the right.

## 2. *On the Influence of Light on the Tadpole.*

The experiments of Dr. Edwards indicate that a decided influence is exerted by light upon the metamorphosis of Batrachians, since, according to his statement, when tadpoles which had arrived at nearly their full growth were secluded from the influence of light, but supported with aerated water and food, they attained an extraordinary size, without undergoing any metamorphosis. The following is Dr. Edwards's experiment:—"I procured a tin box, divided into twelve compartments, each of which was numbered and pierced with holes so that the water might readily pass through the box. A tadpole (which had been previously weighed) was put into each compartment, and the box was then placed in the River Seine, some feet below the surface. A large number were at the same time put into an earthen-ware vessel, containing about four gallons of Seine water, which was changed every day; these tadpoles were at liberty to rise to the surface and respire air, and they soon went through their metamorphosis. Of the twelve placed in the box under water, ten preserved their form without any progress in their transformation, although some had doubled or trebled their weight. It should be observed that at the time when the experiment was begun, the tadpoles had attained the size at which the change is about to take place. Two only were transformed, and these very much later than those which, in the earthen vessel, had the liberty of respiration in air." Dr. Edwards concludes that the presence of solar light favours the development of form.

The situation in which Dr. Edwards placed the tadpoles, "some feet below the surface of the river" in his experiment, would inevitably prove unsuccessful in the full development of the frog. I have always found the transformation, both of the triton and of the frog, equal in the same temperature, both in the light and in the absence of light, if placed in shallow water; but during their metamorphosis they must be allowed to rise to the surface of the water to obtain air, or they become asphyxiated. I therefore placed stones in the vessel, and allowed them to leave the water for the purpose of atmospheric respiration.

The metamorphosis of the tadpole, when at its *full* growth, requires about fourteen days to bring it to the condition of a frog. About the termination of that period, the diminution of the body is so great, and also the absorption of the expanded caudal extremity is such, as to diminish cutaneous respiration. Respiration by the lungs becomes absolutely necessary to prevent the animal from becoming asphyxiated, which would be the case if it remained in the water—requiring then not an aquatic, but an atmospheric medium of respiration. It may be observed that after the tail is partially absorbed, leaving only a portion of the solid part, the asphyxiated state has commenced: the little animal, with open mouth, gasps for breath; but if removed into atmospheric air, the mouth is directly closed, and respiration is effected through the nostrils with perfect freedom; the animal is restored directly, jumps about and is lively.

### 3. *On the Influence of the absence of Light on the Tadpole and on the Frog.*

This time I commenced my experiments in three rock cellars, formerly only in one.

The cellars in Nottingham, cut out of solid rock, are most favourable for experiment; no solar light ever enters, and they are not subject to any great change of temperature. The deepest cellar is 30 feet deep, the mean temperature 51° Fahr.; the middle cellar is 18 feet deep, its mean temperature 53° Fahr.; the uppermost cellar 9 feet deep, mean temperature 56° Fahr.

June 11th. In each cellar I placed a shallow glazed earthenware vessel, containing two pints of water, with grass for chlorophyll for food, changing the water every second day. In each vessel I put twenty tadpoles, approaching the period of their metamorphosis, following the example of Dr. Edwards,—a much easier method than commencing with the spawn.

In the uppermost cellar ten were fully developed in the form of a frog on the 8th of September, and were on the stones, having left the water.

In the middle cellar ten were fully developed on the 22nd of September.

In the lowest cellar eight only had left the water, being fully developed on the 20th of October.

In the following year, July 1st, I made a similar experiment in

the same cellars, three weeks later. The tadpoles were of a large size. I obtained the same result,—the full development of the frog in the absence of light; but in this experiment I had another object in view, that of observing the growth and obtaining the exact weight of the tadpoles before, during, and after their metamorphosis into a frog.

Dr. Edwards said that in his experiment “the tadpoles attained an extraordinary size, doubling or trebling their usual full weight;” but he unfortunately does not mention any particular weight, or how long the tadpoles were preserved alive; in fact there is nothing definite.

During my several years of experiments I did not observe any remarkable increase of weight or size as mentioned by Dr. Edwards, although my first experiment was from the ovum to the full development of the frog, and the two last when the tadpoles were approaching the period of their development.

In my first experiment on the ovum, I never obtained a tadpole more than 8 grains in weight in the absence of light; but I found in a pool in the neighbourhood a number of tadpoles, some between 11 and 15 grains in weight; seven of them weighed 15 grains each. Of these large tadpoles I took twenty for my experiment, weighing altogether 264 grains, and averaging about 13 grains each. After their transformation the frogs weighed 93 grains, averaging about  $4\frac{1}{2}$  grains each,—those of 15 grains in the tadpole state only weighing 5 grains as frogs, having lost two-thirds of their weight during their metamorphosis.

Subsequent experiments have been in accordance with the above.

### III. “Note on Internal Radiation.” By GEORGE G. STOKES, M.A., Sec. R.S., Lucasian Professor of Mathematics in the University of Cambridge. Received December 28, 1861.

In the eleventh volume of the ‘*Proceedings of the Royal Society*,’ p. 193, is the abstract of a paper by Mr. Balfour Stewart, in which he deduces an expression for the internal radiation in any direction within a uniaxial crystal from an equation between the radiations incident upon and emerging from a unit of area of a plane surface, having an arbitrary direction, by which the crystal is supposed to be bounded. With reference to this determination he remarks (p. 196), “But the

internal radiation, if the law of exchanges be true, is clearly independent of the position of this surface, which is indeed merely employed as an expedient. This is equivalent to saying that the constants which define the position of the bounding surface must ultimately disappear from the expression for the internal radiation.\* This anticipation he shows is verified in the case of the expression deduced, according to his principles, for the internal radiation within a uniaxial crystal, on the assumption that the wave-surface\* is the sphere and spheroid of Huygens.

In the case of an uncrystallized medium, the following is the equation obtained by Mr. Stewart in the first instance.

Let  $R, R'$  be the external and internal radiations in directions  $OP, OP'$ , which are connected as being those of an incident and refracted ray, the medium being supposed to be bounded by a plane surface passing through  $O$ . Let  $OP$  describe an elementary conical circuit enclosing the solid angle  $\delta\phi$ , and let  $\delta\phi'$  be the elementary solid angle enclosed by the circuit described by  $OP'$ . Let  $i, i'$  be the angles of incidence and refraction. Of a radiation proceeding along  $PO$ , let the fraction  $A$  be reflected and the rest transmitted; and of a radiation proceeding internally along  $P'O$  let the fraction  $A'$  be reflected, and the rest transmitted. Then by equating the radiation incident externally on a unit of surface, in the directions of lines lying within the conical circuit described by  $OP$ , with the radiation proceeding in a contrary direction, and made up partly of a refracted and partly of an externally reflected radiation, we obtain

$$\begin{aligned} R \cos i \delta\phi &= (1-A') R' \cos i' \delta\phi' + A R \cos i \delta\phi, \\ \text{or} \quad (1-A) R \cos i \delta\phi &= (1-A') R' \cos i' \delta\phi'. \quad \dots \quad (1) \end{aligned}$$

In the case of a crystal there are *two* internal directions of refraction,  $OP_1, OP_2$ , corresponding to a given direction  $PO$  of incidence, the rays along  $OP_1, OP_2$  being each polarized in a particular manner.

\* To prevent possible misapprehension, it may be well to state that I use this term to denote the surface, whatever it may be, which is the locus of the points reached in a given time by a disturbance propagated in all directions from a given point; I do not use it as a name for the surface defined analytically by the equation

$$(x^2 + y^2 + z^2)(a^2 x^2 + b^2 y^2 + c^2 z^2) - a^2(b^2 + c^2)x^2 - b^2(c^2 + a^2)y^2 - c^2(a^2 + b^2)z^2 + a^2 b^2 c^2 = 0.$$

As the term *wave-surface* in its physical signification is much wanted in optics, the surface defined by the above equation should, I think, be called *Fresnel's surface*, or *the wave-surface of Fresnel*.

Conversely, there are two directions,  $P_1O$ ,  $P_2O$ , in which a ray may be incident internally so as to furnish a ray refracted along  $OP$ , and in each case no second refracted ray will be produced, provided the incident ray be polarized in the same manner as the refracted ray  $OP_1$  or  $OP_2$ . In the case of a crystal, then, equation (1) must be replaced by

$$(1-A) R \cos i \delta \phi = (1-A_1) R_1 \cos i_1 \delta \phi_1 + (1-A_2) R_2 \cos i_2 \delta \phi_2. \quad (2)$$

In the most general case it does not appear in what manner, if at all, equation (2) would split into two equations, involving respectively  $R_1$  and  $R_2$ . For if an incident ray  $PO$  were so polarized as to furnish only one refracted ray, say  $OP_1$ , a ray incident along  $P_1O$  and polarized in the same manner as  $OP_1$  would furnish indeed only one refracted ray, in the direction  $OP$ , but that would be polarized differently from  $PO$ ; so that the two systems are mixed up together.

But if the plane of incidence be a principal plane, and if we may assume that such a plane is a plane of symmetry as regards the optical properties of the medium\*, the system of rays polarized in and the system polarized perpendicularly to the plane of incidence will be quite independent of each other, and the equality between the radiation incident externally and that proceeding in the contrary direction, and made up partly of a refracted and partly of an externally reflected radiation, must hold good for each system separately. In this case, then, (2) will split into two equations, each of the form (1),  $R$  now standing for half the whole radiation, and  $R'$ ,  $A'$ , &c. standing for  $R_1$ ,  $A_1$ , &c., or  $R_2$ ,  $A_2$ , &c., as the case may be. It need hardly be remarked that the value of  $A$  is different in the two cases, and that  $R'$  has a value which is no longer, as in the case of an isotropic medium, alike in all

\* According to Sir David Brewster (Report of the British Association for 1836, part ii. p. 13, and for 1842, part ii. p. 13), when light is incident on a plane surface of Iceland spar in a plane parallel to the axis, the plane of incidence, which is a principal plane, is not in general a plane of optical, any more than of crystalline symmetry as regards the phenomena of reflexion, although, as is well known, all planes passing through the axis are alike as regards internal propagation and the polarization of the refracted rays. Hence, strictly speaking, the statement as to the independence of the two systems of rays should be confined to the case in which the principal plane is also a plane of crystalline symmetry. As, however, the unsymmetrical phenomena were only brought out when the ordinary reflexion was weakened, almost annihilated, by the use of oil of cassia, we may conclude that under common circumstances they would be insensible.



gards the extraordinary, it is by no means obvious that the ratio should be expressible in the form indicated—as a quantity depending only on the direction  $OP'$ . Mr. Stewart has, however, proved that this is the case, independently of any restriction as to the plane of incidence being a principal plane, on the assumption that the wave-surface has the form assigned to it by Huygens.

It might seem at first sight that this verification was fairly adducible in confirmation of the truth of the whole theory, including the assumed form of the wave-surface. But a little consideration will show that such a view cannot be maintained. Huygens's construction links together the law of refraction and the form of the wave-surface, in a manner depending for its validity only on the most fundamental principles of the theory of undulations. The construction which Huygens applied to the ellipsoid is equally applicable to any other surface; it was a mere guess on his part that the extraordinary wave-surface in Iceland spar was an ellipsoid; and although the ellipsoidal form results from the imperfect dynamical theory of Fresnel, it is certain that rigorous dynamical theories lead to different forms of the wave-surface, according to the suppositions made as to the existing state of things. For every such possible form the ratio expressed by the right-hand member of equation (3) ought to come out in the form indicated by the left-hand member, and not to involve explicitly the direction of the refracting plane: and as it seemed evident that it could not be possible, merely by such general considerations as those adduced by Mr. Stewart, to distinguish between those surfaces which were and those which were not dynamically possible forms of the wave-surface, I was led to anticipate that the possibility of expressing the ratio in question under the form indicated was a general property of surfaces. The object of the present Note is to give a demonstration of the truth of this anticipation, and thereby remove from the verification the really irrelevant consideration of a particular form of wave-surface; but it was necessary in the first instance to supply some steps of Mr. Stewart's investigation which are omitted in the published abstract.

The proposition to be proved may be somewhat generalized, in a manner suggested by the consideration of internal reflexion within a crystal, or refraction out of one crystallized medium into another in optical contact with it. Thus generalized it stands as follows:—

Imagine any two surfaces whatsoever, and also a fixed point  $O$ ;



imagine likewise a plane  $\Pi$  passing through  $O$ . Let two points  $P, P'$ , situated on the two surfaces respectively, and so related that the tangent planes at those points intersect each other in the plane  $\Pi$ , be called *corresponding points with respect to the plane  $\Pi$* . Let  $P$  describe, on the surface on which it lies, an infinitesimal closed circuit, and  $P'$  the "corresponding" circuit; let  $\delta\phi, \delta\phi'$  be the solid angles subtended at  $O$  by these circuits respectively, and  $i, i'$  the inclinations of  $OP, OP'$  to the normal to  $\Pi$ . Then shall the ratio of  $\cos i\delta\phi$  to  $\cos i'\delta\phi'$  be of the form  $[P] : [P']$ , where  $P$  depends only on the first surface and the position of  $P$ , and  $[P']$  only on the second surface and the position of  $P'$ . Moreover, if either surface be a sphere having its centre at  $O$ , the corresponding quantity  $[P]$  or  $[P']$  shall be constant.

It may be remarked that the two surfaces may be merely two sheets of the same surface, or even two different parts of the same sheet.

Instead of comparing the surfaces directly with each other, it will be sufficient to compare them both with the same third surface; for it is evident that if the points  $P, P'$  correspond to the same point  $P_1$ , on the third surface, they will also correspond to each other. For the third surface it will be convenient to take a sphere described round  $O$  as centre with an arbitrary radius, which we may take for the unit of length. The letters  $P_1, i_1, \phi_1$  will be used with reference to the sphere.

Let the surface and sphere be referred to rectangular coordinates,  $O$  being the origin, and  $\Pi$  the plane of  $xy$ . Let  $x, y, z$  be the coordinates of  $P$ ;  $\xi, \eta, \zeta$  those of  $P_1$ . Then  $x, y, z$  will be connected by the equation of the surface, and  $\xi, \eta, \zeta$  by the equation

$$\xi^2 + \eta^2 + \zeta^2 = 1.$$

According to the usual notation, let

$$\frac{dz}{dx} = p, \quad \frac{dz}{dy} = q, \quad \frac{d^2z}{dx^2} = r, \quad \frac{d^2z}{dxdy} = s, \quad \frac{d^2z}{dy^2} = t.$$

The equations of the tangent planes at  $P, P_1, X, Y, Z$  being the current coordinates, are

$$\begin{aligned} Z - z &= p(X - x) + q(Y - y), \\ \xi X + \eta Y + \zeta Z &= 1; \end{aligned}$$

and those of their traces on the plane of  $xy$  are

$$\begin{aligned} pX + qY &= px + qy - z, \\ \xi X + \eta Y &= 1; \end{aligned}$$

and in order that these may represent the same line, we must have

$$\xi = \frac{p}{px + qy - s}, \quad \eta = \frac{q}{px + qy - s}. \quad (4)$$

To the element  $dx dy$  of the projection on the plane of  $xy$  of a superficial element at P, belongs the superficial element  $dS = \sqrt{1 + p^2 + q^2} dx dy$ , and to this again belongs the elementary solid angle  $\frac{\cos \nu dS}{\rho^2}$ , where  $\rho = OP$ , and  $\nu$  is the angle between the normal at P and the radius vector. Hence the total solid angle within a small contour is  $\frac{\cos \nu}{\rho^2} \sqrt{1 + p^2 + q^2} \iint dx dy$ , the double integral being taken within the projection of that small contour. Also  $\cos i = \frac{z}{\rho}$ . Hence

$$\cos i \delta\phi = \frac{z \cos \nu}{\rho^2} \sqrt{1 + p^2 + q^2} \iint dx dy;$$

and applying this formula to the sphere by replacing  $z \sqrt{1 + p^2 + q^2}$  by 1,  $\nu$  by 0, and  $\rho$  by 1, we have

$$\cos i_1 \delta\phi_1 = \iint d\xi d\eta,$$

the double integral being taken over the projection of the corresponding small area of the sphere.

Now by the well-known formula for the transformation of multiple integrals we have

$$\iint d\xi d\eta = \iint \left( \frac{d\xi}{dx} \frac{d\eta}{dy} - \frac{d\xi}{dy} \frac{d\eta}{dx} \right) dx dy;$$

and therefore

$$\frac{\cos i \delta\phi}{\cos i_1 \delta\phi_1} = \frac{z \cos \nu \sqrt{1 + p^2 + q^2}}{\rho^2 \left( \frac{d\xi}{dx} \frac{d\eta}{dy} - \frac{d\xi}{dy} \frac{d\eta}{dx} \right)}.$$

But the first of equations (4) gives

$$\begin{aligned} d\xi &= \frac{(px + qy - s)dp - p(xdp + ydq)}{(px + qy - s)^2} \\ &= \frac{\{(qy - s)r - py s\} dx + \{(qy - s)s - py t\} dy}{(px + qy - s)^2}. \end{aligned}$$

Similarly,

$$d\eta = \frac{\{(px - s)t - qxs\} dy + \{(px - s)s - qxr\} dx}{(px + qy - s)^2}.$$

Hence

$$\frac{d\xi}{dx} \frac{d\eta}{dy} - \frac{d\xi}{dy} \frac{d\eta}{dx} = \frac{V}{(px + qy - z)^2},$$

where

$$\begin{aligned} V &= \{(qy - z)r - pzs\} \{(px - z)t - qxs\} - \{(qy - z)s - pyt\} \{(px - z)s - qxr\} \\ &= \{(qy - z)(px - z) - pqxy\} (rt - s^2) \\ &= z(z - px - qy)(rt - s^2). \end{aligned}$$

Hence

$$\frac{\cos i \delta \phi}{\cos i_1 \delta \phi_1} = \frac{z \cos \nu \sqrt{1 + p^2 + q^2} (z - px - qy)^2}{\rho^3 z (rt - s^2)}.$$

But if  $\omega$  be the perpendicular let fall from O on the tangent plane at P,

$$z - px - qy = \sqrt{1 + p^2 + q^2} \cdot \omega,$$

and therefore

$$\frac{\cos i \delta \phi}{\cos i_1 \delta \phi_1} = \frac{\cos \nu \cdot \omega^2}{\rho^3} \frac{(1 + p^2 + q^2)^2}{rt - s^2}.$$

But  $\omega = \rho \cos \nu$ . Also the quadratic determining the principal radii of curvature at P is

$$(rt - s^2)v^2 + (\&c.)v + (1 + p^2 + q^2)^2 = 0;$$

and therefore if  $v_1, v_2$  denote the principal radii of curvature,

$$v_1 v_2 = \frac{(1 + p^2 + q^2)^2}{rt - s^2}.$$

Hence

$$\frac{\cos i \delta \phi}{\cos i_1 \delta \phi_1} = \cos^4 \nu \cdot v_1 v_2 \quad \dots \dots \dots (5)$$

and

$$\frac{\cos i \delta \phi}{\cos i' \delta \phi'} = \frac{\cos i \delta \phi}{\cos i_1 \delta \phi_1} \cdot \frac{\cos i_1 \delta \phi_1}{\cos i' \delta \phi'} = \frac{\cos^4 \nu \cdot v_1 v_2}{\cos^4 \nu' \cdot v_1' v_2'} \quad \dots \dots \dots (6)$$

which proves the proposition enunciated.

In the particular case of an ellipsoid of revolution of which  $n$  is the axial and  $m$  the equatorial semi-axis, compared with a sphere of radius unity, both having their centres at O', one of the principal radii of curvature is the normal of the elliptic section, which by the properties of the ellipse is equal to  $\frac{m}{n} m'$ ,  $m'$  denoting the semi-conjugate diameter; and the other is the radius of curvature of the elliptic section, or  $\frac{m^3}{mn}$ . Also  $\omega$  is the perpendicular let fall from the centre

on the tangent line of the section. Hence from (5) or (6)

$$\frac{\cos i \delta \phi}{\cos i' \delta \phi} = \frac{w^4}{\rho^4} \cdot \frac{mm'}{n} \cdot \frac{m'^3}{mn} = \frac{w^4 m'^4}{n^2 \rho^4} = \frac{m^4 n^2}{\rho^4},$$

since  $wm' = mn$ . This agrees with Mr. Stewart's result (p. 197), since the  $R_e$  and  $\frac{1}{2}R$  of Mr. Stewart are the same as the  $R'$  and  $R$  of equation (3).

IV. "On the Intensity of the Light reflected from or transmitted through a Pile of Plates." By GEORGE G. STOKES, M.A., Sec. R.S., Lucasian Professor of Mathematics in the University of Cambridge. Received January 1, 1862.

The frequent employment of a pile of plates in experiments relating to polarization suggests, as a mathematical problem of some interest, the determination of the mode in which the intensity of the reflected light, and the intensity and degree of polarization of the transmitted light, are related to the number of the plates, and, in case they be not perfectly transparent, to their defect of transparency.

The plates are supposed to be bounded by parallel surfaces, and to be placed parallel to one another. They will also be supposed to be formed of the same material, and to be of equal thickness, except in the case of perfect transparency, in which case the thickness does not come into account. The plates themselves and the interposed plates of air will be supposed, as is usually the case, to be sufficiently thick to prevent the occurrence of the colours of thin plates, so that we shall have to deal with intensities only.

On account of the different proportions in which light is reflected at a single surface according as the light is polarized in or perpendicularly to the plane of incidence, we must take account separately of light polarized in these two ways. Also, since the rate at which light is absorbed varies with its refrangibility, we must take account separately of the different constituents of white light. If, however, the plates be perfectly transparent, we may treat white light as a whole, neglecting as insignificant the chromatic variations of reflecting power. Let  $\rho$  be the fraction of the incident light reflected at the first surface of a plate. Then  $1 - \rho$  may be taken as the intensity of

the transmitted light\*. Also, since we know that light is reflected in the same proportion externally and internally at the two surfaces of a plate bounded by parallel surfaces, the same expressions  $\rho$  and  $1 - \rho$  will serve to denote the fractions reflected and transmitted at the second surface. We may calculate  $\rho$  in accordance with Fresnel's formulæ from the expressions

$$\sin i' = \frac{\sin i}{\mu}, \quad . . . . . (1)$$

$$\rho = \frac{\sin^2(i - i')}{\sin^2(i + i')} \text{ or } = \frac{\tan^2(i - i')}{\tan^2(i + i')}, \quad . . . . . (2)$$

according as the light is polarized in or perpendicularly to the plane of incidence.

In the case of perfect transparency, we may in imagination make abstraction of the substance of the plates, and state the problem as follows:—There are  $2m$  parallel surfaces ( $m$  being the number of plates) on which light is incident, and at each of which a given fraction  $\rho$  of the light incident upon it is reflected, the remainder being transmitted; it is required to determine the intensity of the light reflected from or transmitted through the system, taking account of the reflexions, infinite in number, which can occur in all possible ways.

This problem, the solution of which is of a simpler form than that of the general case of imperfect transparency, might be solved by a particular method. As, however, the solution is comprised in that of the problem which arises when the light is supposed to be partially absorbed, I shall at once pass on to the latter.

In consequence of absorption, let the intensity of light traversing a plate be reduced in the proportion of 1 to  $1 - qdx$  in passing over the elementary distance  $dx$  within the plate. Let  $T$  be the thickness of a plate, and therefore  $T \sec i'$  the length of the path of the light within it. Then, putting for shortness

$$e^{-qT \sec i'} = g, \quad . . . . . (3)$$

1 to  $g$  will be the proportion in which the intensity is reduced by

\* In order that the intensity may be measured in this simple way, which saves trouble in the problem before us, we must define the intensity of the light transmitted across the first surface to mean what would be the intensity if the light were to emerge again into air across the second surface without suffering loss by absorption, or by reflexion at that surface.

absorption in a single transit. The light reflected by a plate will be made up of that which is reflected at the first surface, and that which suffers 1, 3, 5, &c. internal reflexions. If the intensity of the incident light be taken as unity, the intensities of these various portions will be

$$\rho, (1-\rho)^2 \rho g^2, (1-\rho)^4 \rho^3 g^4, \&c.$$

and if  $r$  be the intensity of the reflected light, we have, by summing a geometric series,

$$r = \rho + \frac{(1-\rho)^2 \rho g^2}{1-\rho^2 g^2} \quad \dots \quad (4)$$

Similarly, if  $t$  be the intensity of the transmitted light,

$$t = \frac{(1-\rho)g}{1-\rho^2 g^2}, \quad \dots \quad (5)$$

and we easily find

$$r = \rho + g\rho t; \quad r + t = 1 - \frac{(1-\rho)(1-g)}{1-\rho g},$$

which is in general less than 1, but becomes equal to 1 in the limiting case of perfect transparency, in which case  $g=1$ .

The values of  $\mu$ ,  $i$ , and  $q$  in any case being supposed known, the formulæ (1), (2), (3), (4), (5) determine  $r$  and  $t$ , which may now therefore be supposed known. The problem therefore is reduced to the following:—There are  $m$  parallel plates of which each reflects and transmits given fractions  $r$ ,  $t$  of the light incident upon it: light of intensity unity being incident on the system, it is required to find the intensities of the reflected and refracted light.

Let these be denoted by  $\phi(m)$ ,  $\psi(m)$ . Consider a system of  $m+n$  plates, and imagine these grouped into two systems, of  $m$  and  $n$  plates respectively. The incident light being represented by unity, the light  $\phi(m)$  will be reflected from the first group, and  $\psi(m)$  will be transmitted. Of the latter the fraction  $\psi(n)$  will be transmitted by the second group, and  $\phi(n)$  reflected. Of the latter the fraction  $\psi(m)$  will be transmitted by the first group, and  $\phi(m)$  reflected, and so on. Hence we get for the light reflected by the whole system,

$$\phi(m) + (\psi m)^2 \phi(n) + (\psi m)^2 \phi(m) (\phi n)^2 + \dots,$$

and for the light transmitted,

$$\psi(m)\psi(n) + \psi(m)\phi(n)\phi(m)\psi(n) + \psi(m)(\phi n)^2(\phi m)^2\psi(n) + \dots,$$

which gives, by summing the two geometric series, .

$$\phi(m+n) = \phi(m) + \frac{(\psi m)^2 \phi(n)}{1 - \phi(m)\phi(n)}; \quad . \quad . \quad . \quad (6)$$

$$\psi(m+n) = \frac{\psi(m)\psi(n)}{1 - \phi(m)\phi(n)}. \quad . \quad . \quad . \quad (7)$$

We get from (6)

$$\phi(m+n)\{1 - \phi(m)\phi(n)\} = \phi(m) + \phi(n)\{(\psi m)^2 - (\phi m)^2\};$$

and the first member of this equation being symmetrical with respect to  $m$  and  $n$ , we get, by interchanging  $m$  and  $n$  and equating the results,

$$\phi(m) + \phi(n)\{(\psi m)^2 - (\phi m)^2\} = \phi(n) + \phi(m)\{(\psi n)^2 - (\phi n)^2\};$$

or

$$\frac{1}{\phi(m)}\{1 + (\phi m)^2 - (\psi m)^2\} = \frac{1}{\phi(n)}\{1 + (\phi n)^2 - (\psi n)^2\},$$

which is therefore constant. Denoting this constant for convenience by  $2 \cos \alpha$ , we have

$$(\psi m)^2 = 1 - 2 \cos \alpha \cdot \phi(m) + (\phi m)^2. \quad . \quad . \quad . \quad (8)$$

Squaring (7), and eliminating the function  $\psi$  by means of (8), we find

$$\begin{aligned} \{1 - \phi(m)\phi(n)\}^2 \{1 - 2 \cos \alpha \cdot \phi(m+n) + [\phi(m+n)]^2\} \\ = \{1 - 2 \cos \alpha \cdot \phi(m) + (\phi m)^2\} \{1 - 2 \cos \alpha \cdot \phi(n) + (\phi n)^2\}. \end{aligned} \quad (9)$$

From the nature of the problem,  $m$  and  $n$  are positive integers, and it is only in that case that the functions  $\phi$ ,  $\psi$ , as hitherto defined, have any meaning. We may, however, contemplate functions  $\phi$ ,  $\psi$  of a continuously changing variable, which are defined by the equations (6) and (7); and it is evident that if we can find such functions, they will in the particular case of a positive integral value of the variable be the functions which we are seeking.

In order that equations (6), (7) may hold good for a value zero of one of the variables, suppose  $n$ , we must have  $\phi(0)=0$ ,  $\psi(0)=1$ . The former of these equations reduces (9) for  $n=0$  to an identical equation. Differentiating (9) with respect to  $n$ , and after differentiation putting  $n=0$ , we find

$$\begin{aligned} \phi'(0)\phi(m)\{1 - 2 \cos \alpha \cdot \phi(m) + (\phi m)^2\} + \cos \alpha \cdot \phi'(m) - \phi(m)\phi'(m) \\ = \cos \alpha \cdot \phi'(0)\{1 - 2 \cos \alpha \cdot \phi(m) + (\phi m)^2\}, \end{aligned}$$

or dividing out by  $\phi(m) - \cos \alpha$ , (for  $\phi(m) = \cos \alpha$  would only lead to  $\phi(m) = \cos \alpha = 0$ ,  $\psi(m) = C$ ),

$$\phi'(m) = \phi'(0) \{1 - 2 \cos \alpha \phi(m) + (\psi(m))^2\}. \quad (10)$$

Integrating this equation, determining the arbitrary constant by the condition that  $\phi(m) = 0$  when  $m = 0$ , and writing  $\beta$  for  $\sin \alpha \cdot \phi'(0)$ , we have

$$\phi(m) = \frac{\sin m\beta}{\sin(\alpha + m\beta)}. \quad (11)$$

Substituting in (8) and reducing, we find

$$(\psi(m))^2 = \frac{\sin^2 \alpha}{\sin^2(\alpha + m\beta)}. \quad (12)$$

But (8) was derived, not from (7) directly, but from (7) squared; and on extracting the square root of both sides of (12), we must choose that sign which shall satisfy (7), and therefore we must take the sign +, as we see at once on putting  $m = n = 0$ . The equation (12) on taking the proper root and (11) may be put under the form

$$\frac{\phi(m)}{\sin(m\beta)} = \frac{\psi(m)}{\sin \alpha} = \frac{1}{\sin(\alpha + m\beta)}; \quad (13)$$

and to determine the arbitrary constants  $\alpha, \beta$  we have, putting  $m = 1$ , and  $\phi(m) = r$ ,  $\psi(m) = t$ ,

$$\frac{r}{\sin \beta} = \frac{t}{\sin \alpha} = \frac{1}{\sin(\alpha + \beta)}. \quad (14)$$

We readily get from equations (13),

$$1 - \phi(m)\phi(n) = \frac{\sin \alpha \sin \{\alpha + (m+n)\beta\}}{\sin(\alpha + m\beta) \sin(\alpha + n\beta)};$$

$$\phi(m+n) - \phi(m) = \frac{\sin \alpha \sin n\beta}{\sin \{\alpha + (m+n)\beta\} \sin(\alpha + m\beta)};$$

whence the equations (6), (7) are easily verified. This verification seems necessary in logical strictness, because we have no right to assume *a priori* that it is possible to satisfy (6) and (7) for general values of the variables; and in deriving the equation (10), the equations (6) and (7) were only assumed to hold good for general values of  $m$  and infinitely small values of  $n$ .

The equations (13), (14) give the following *quasi*-geometrical construction for solving the problem:—Construct a triangle of which



the sides represent in magnitude the intensity of the incident, reflected, and refracted light in the case of a single plate, and then, leaving the first side and the angle opposite to the third unchanged, multiply the angle opposite to the second by the number of plates; the sides of the new triangle will represent the corresponding intensities in the case of the system of plates. I say *quasi*-geometrical, because the construction cannot actually be effected, inasmuch as the first side of our triangle is greater than the sum of the two others, and the angles are imaginary.

To adapt the formulæ (13), (14) to numerical calculation, it will be convenient to get rid of the imaginary quantities. Putting

$$\sqrt{\{(1+r+t)(1+r-t)(1+t-r)(1-r-t)\}} = \Delta, \quad (15)$$

we have by the common formulæ of trigonometry,

$$\cos \alpha = \frac{1+r^2-t^2}{2r}; \quad \sin \alpha = \frac{\pm \sqrt{-1}\Delta}{2r};$$

whence, putting

$$\frac{1}{2r}(1+r^2-t^2+\Delta) = a, \quad (16)$$

we have

$$e^{\sqrt{-1}\alpha} = \cos \alpha + \sqrt{-1} \sin \alpha = a^{\mp 1}.$$

It is a matter of indifference which sign be taken: choosing the under signs, we have

$$2r \sin \alpha = -\sqrt{-1}\Delta, \quad e^{\sqrt{-1}\alpha} = a.$$

We have also

$$\cos \beta = \frac{1+t^2-r^2}{2t}, \quad \sin \beta = \frac{r}{t} \sin \alpha = -\frac{\sqrt{-1}\Delta}{2t},$$

no fresh ambiguity of sign being introduced. Putting therefore

$$\frac{1}{2t}(1+t^2-r^2+\Delta) = b, \quad (17)$$

we have

$$e^{\sqrt{-1}\beta} = b;$$

and equations (13) now give

$$\frac{\phi(m)}{b^m - b^{-m}} = \frac{\psi(m)}{a - a^{-1}} = \frac{1}{ab^m - a^{-1}b^{-m}} \quad (18)$$

In the case of perfect transparency these expressions take a sim-

pler form. If  $r+t$  differ indefinitely little from 1,  $\alpha$  and  $\beta$  will be indefinitely small. Making  $\alpha$  and  $\beta$  indefinitely small in (13) and (14), and putting  $1-r$  for  $t$ , we find

$$\frac{\phi(m)}{mr} = \frac{\psi(m)}{1-r} = \frac{1}{1+(m-1)r} \quad \dots \quad (19)$$

In this case it is evident that each of the  $2m$  reflecting surfaces might be regarded as a separate plate reflecting light in the proportion of  $\rho$  to 1, and therefore we ought also to have, writing  $2m$  for  $m$  and  $\rho$  for  $r$  in the denominators of the equations (19),

$$\frac{\phi(m)}{2m\rho} = \frac{\psi(m)}{1-\rho} = \frac{1}{1+(2m-1)\rho} \quad \dots \quad (20)$$

It is easy to verify that when  $g=1$  (4) reduces (19) to (20).

The following Table gives the intensity of the light reflected from or transmitted through a pile of  $m$  plates for the values 1, 2, 4, 8, 16, 32, and  $\infty$  of  $m$ , for three degrees of transparency, and for certain selected angles of incidence. The assumed refractive index  $\mu$  is 1.52.  $\delta=1-e^{-\tau T}$  is the loss by absorption in a single transit of a plate at a perpendicular incidence, so that  $\delta=0$  corresponds to perfect transparency. The most interesting angles of incidence to select appeared to be zero and the polarizing angle  $\omega=\tan^{-1} \mu$ ; but in the case of perfect transparency the result has also been calculated for an angle of incidence a little ( $2^\circ$ ) greater than the polarizing angle.  $\phi$  denotes the intensity of the reflected and  $\psi$  that of the transmitted light, the intensity of the incident light being taken at 1000. For oblique incidences it was necessary to distinguish between light polarized in and light polarized perpendicularly to the plane of incidence; the suffixes 1, 2 refer to these two kinds respectively. For oblique incidences a column is added giving the ratio of  $\psi_1$  to  $\psi_2$ , which may be taken as a measure of the defect of polarization of the transmitted light. No such column was required for  $\delta=0$  and  $i=\omega$ , because in this case  $\psi_2=1000$ .

\* From a paper by M. Wild in Poggendorff's 'Annalen' [vol. ix. (1856) p. 240], I find that the formulæ for the particular case of perfect transparency have already been given by M. Neumann. His demonstration does not appear to have been published.

$\delta=0$										$\delta=\frac{\pi}{2}$						$\delta=\pi$							
$i=0$			$i=w$			$i=w+2^\circ$				$i=0$			$i=w$			$i=0$			$i=w$				
$\phi$	$\psi$	$\frac{\psi_1}{\psi_2}$	$\phi_1$	$\psi_1$	$\phi_2$	$\psi_2$	$\phi_1$	$\psi_1$	$\phi_2$	$\psi_2$	$\frac{\psi_1}{\psi_2}$	$\phi$	$\psi$	$\frac{\psi_1}{\psi_2}$	$\phi_1$	$\psi_1$	$\phi_2$	$\psi_2$	$\frac{\psi_1}{\psi_2}$	$\phi$	$\psi$	$\frac{\psi_1}{\psi_2}$	
1	82	918	271	729	300	700	1	999	701	300	701	80	900	265	711	976	728	74	826	245	639	881	725
2	151	849	426	574	459	541	2	998	542	459	542	145	815	410	544	953	571	125	686	351	435	777	559
4	262	738	598	402	628	372	4	996	373	628	373	244	679	555	355	908	391	185	479	427	215	604	357
8	416	584	749	251	771	229	8	992	231	870	130	364	490	656	182	824	221	229	237	451	57	365	156
16	587	413	856	144	870	130	16	984	132	931	69	464	276	695	58	679	086	243	59	453	4	133	030
32	740	260	922	78	931	69	32	968	071	1000	0	509	97	699	7	461	014	244	4	453	0	18	001
$\infty$	1000	0	1000	0	1000	0	1000	0	1000	0	000	516	0	699	0	0	000	244	0	453	0	0	000

The intensity of the light reflected from an infinite number of plates, as we see from (18), is  $a^{-1}$ ; and since  $a$  is changed into  $a^{-1}$  by changing the sign of  $\alpha$  or of  $\Delta$ ,

$$a^{-1} = \frac{1}{2r}(1 + r^2 - t^2 - \Delta), \quad . \quad . \quad . \quad . \quad (21)$$

which is equal to 1 in the case of perfect transparency. Accordingly a substance which is at the same time finely divided, so as to present numerous reflecting surfaces, and which is of such a nature as to be transparent in mass, is brilliantly white by reflected light,—for example snow, and colourless substances thrown down as precipitates in chemical processes.

The intensity of the light reflected from a pile consisting of an infinite number of similar plates falls off rapidly with the transparency of the material of which the plates are composed, especially at small incidence. Thus at a perpendicular incidence we see from the above Table that the reflected light is reduced to little more than one half when 2 per cent. is absorbed in a single transit, and to less than a quarter when 10 per cent. is absorbed.

With imperfectly transparent plates, little is gained by multiplying the plates beyond a very limited number, if the object be to obtain light, as bright as may be, polarized by reflexion. Thus the Table shows that 4 plates of the less defective kind reflect 79 per cent., and 4 plates of the more defective as much as 94 per cent., of the light that could be reflected by a greater number, whereas 4 plates of the perfectly transparent kind reflect only 60 per cent.

The Table shows that while the amount of light transmitted at the polarizing angle by a pile of a considerable number of plates is materially reduced by a defect of transparency, its state of polarization is somewhat improved. This result might be seen without calculation. For while no part of the transmitted light which is polarized perpendicularly to the plane of incidence underwent reflexion, a large part of the transmitted light polarized the other way was reflected an even number of times; and since the length of path of the light within the absorbing medium is necessarily increased by reflexion, it follows that a defect of transparency must operate more powerfully in reducing the intensity of light polarized in, than of light polarized perpendicularly to the plane of polarization. But the Table also shows that a far better result can be obtained, as to the perfection of the polari-

sation of the transmitted light, without any greater loss of illumination, by employing a larger number of plates of a more transparent kind.

Let us now confine our attention to perfectly transparent plates, and consider the manner in which the degree of polarization of the transmitted light varies with the angle of incidence.

The degree of polarization is expressed by the ratio of  $\psi_1$  to  $\psi_2$ , which for brevity will be denoted by  $\chi$ . When  $\chi=1$  there is no polarization; when  $\chi=0$  the polarization is perfect, in a plane perpendicular to the plane of incidence. Now  $\psi$  (which is used to denote  $\psi_1$  or  $\psi_2$  as the case may be) is given in terms of  $\rho$  by one of the equations (20), and  $\rho$  is given in terms of  $i-i'$  and  $i+i'$  by Fresnel's formulæ (2). Put

$$i-i'=\theta, \quad i+i'=\sigma;$$

then, from (1),

$$\frac{di}{\tan i} = \frac{di'}{\tan i'} = \frac{d\theta}{\tan i - \tan i'} = \frac{d\sigma}{\tan i + \tan i'} = \cos i \cos i' d\omega, \text{ suppose,}$$

whence

$$d\theta = \sin \theta d\omega, \quad d\sigma = \sin \sigma d\omega; \quad . \quad . \quad . \quad (22)$$

and we see that  $i$  and  $\omega$  increase together from  $i=0$  to  $i=\frac{\pi}{2}$ . We

have also

$$\begin{aligned} \rho_1 &= \frac{\sin^2 \theta}{\sin^2 \sigma}, \quad d\rho_1 = \frac{2 \sin \theta}{\sin^2 \sigma} (\sin \sigma \cos \theta d\theta - \sin \theta \cos \sigma d\sigma) = \frac{2 \sin^2 \theta}{\sin^2 \sigma} (\cos \theta - \cos \sigma) d\omega; \\ \rho_2 &= \frac{\tan^2 \theta}{\tan^2 \sigma}, \quad d\rho_2 = \frac{2 \tan \theta}{\tan^2 \sigma} (\tan \sigma \sec^2 \theta d\theta - \tan \theta \sec^2 \sigma d\sigma) \\ &= \frac{2 \sin^2 \theta \cos \sigma}{\cos^3 \theta \sin^2 \sigma} (\cos \sigma - \cos \theta) d\omega = -\frac{\cos \sigma}{\cos^3 \theta} d\rho_1. \end{aligned}$$

Now  $\cos \theta - \cos \sigma$  or  $2 \sin i \sin i'$  is positive; and  $\cos \sigma$  is positive from  $i=0$  to  $i=\omega$ , and negative from  $i=\omega$  to  $i=\frac{\pi}{2}$ . But (20) shows that  $\psi$  decreases as  $\rho$  increases. From  $i=0$  to  $i=\omega$ ,  $\rho_1$  increases and  $\rho_2$  decreases, and therefore  $\psi_1$  decreases and  $\psi_2$  increases, and therefore on both accounts  $\chi$  decreases. When  $i=\omega$ ,  $\frac{d\rho_1}{di}$  is still positive, and therefore  $\frac{d\psi_1}{di}$  negative, but  $\psi_2$  has its maximum value 1, so that on passing through the polarizing angle  $\chi$  still decreases, or the polari-

zation improves. When the plates are very numerous,  $\psi_2=1$  at the polarizing angle, and on both sides of it decreases rapidly, whereas  $\psi_1$ , which is always small, suffers no particular change about the polarizing angle. Hence in this case  $\chi$  must be a minimum a little beyond the polarizing angle. Let us then seek the angle of incidence which makes  $\chi$  a minimum in the case of an arbitrary number of plates.

We have from (20) and (2),

$$\begin{aligned}\chi &= \frac{\sin^2 \sigma - \sin^2 \theta}{\sin^2 \sigma + (2m-1) \sin^2 \theta} \cdot \frac{\sin^2 \sigma \cos^2 \theta + (2m-1) \sin^2 \theta \cos^2 \sigma}{\sin^2 \sigma \cos^2 \theta - \sin^2 \theta \cos^2 \sigma} \\ &= \frac{\sin^2 \sigma \cos^2 \theta + (2m-1) \sin^2 \theta \cos^2 \sigma}{\sin^2 \sigma + (2m-1) \sin^2 \theta} = 1 - \frac{2m}{\operatorname{cosec}^2 \theta + (2m-1) \operatorname{cosec}^2 \sigma}. \quad (23)\end{aligned}$$

Hence  $\chi$  is a minimum along with  $\operatorname{cosec}^2 \theta + (2m-1) \operatorname{cosec}^2 \sigma$ . Differentiating, and taking account of the formulæ (22), we find, to determine the angle of maximum polarization, the very simple equation

$$\cos \theta \sin^2 \sigma + (2m-1) \cos \sigma \sin^2 \theta = 0. \quad (24)$$

For any assumed value of  $i$  from  $\omega$  to  $\frac{\pi}{2}$  this equation gives at once the value of  $m$ , that is, the number of plates of which a pile must be composed in order that the assumed incidence may be that of maximum polarization of the transmitted light. The equation may be put under the form

$$2m-1 = -\frac{\tan \sigma}{\tan \theta} \cdot \frac{\sin \sigma}{\sin \theta} = \frac{1}{\sqrt{\rho_1 \rho_2}}.$$

Now we have seen that both  $\rho_1$  and  $\rho_2$  continually increase, and therefore  $m$  continually decreases, from  $i=\omega$  to  $i=\frac{\pi}{2}$ . At the first of these limits  $\rho_2=0$ , and therefore  $m=\infty$ . At the second  $\rho_1=\rho_2=1$ , and therefore  $m=1$ . Hence with a single plate the polarization of the transmitted light continually improves up to a grazing incidence, but with a pile of plates the polarization attains a maximum at an angle of incidence which approaches indefinitely to the polarizing angle as the number of plates is indefinitely increased.

Eliminating  $m$  from (23) and (24), we find

$$\chi = -\cos \theta \cos \sigma, \quad (25)$$

which determines for any pile  $\chi_1$ , the defect of maximum polarization of the transmitted light, in terms of the angle of incidence for

which the polarization is a maximum. We have, from (25), (22), and (24),

$d\chi_1 = (\sin^2 \theta \cos \sigma + \sin^2 \sigma \cos \theta) d\omega = -2(m-1) \cos \sigma \sin^2 \theta d\omega$ ,  
and  $\cos \sigma$  is negative. Hence  $\chi_1$  decreases as  $\omega$  (and therefore  $i$ ) decreases, or as  $m$  increases. For  $m=1$ ,  $i=\frac{\pi}{2}$  and  $\chi_1=\mu^{-2}$ ; for  $m=\infty$ ,  $\cos \sigma=0$ , and therefore  $\chi_1=0$ , or the maximum polarization tends indefinitely to become perfect as the number of plates is indefinitely increased.

For a given number of plates the angle of maximum polarization may be readily found from (24) by the method of trial and error. But for merely examining the progress of the functions, instead of tabulating  $i$  for assumed values of  $m$ , it will serve equally well to tabulate  $m$  for assumed values of  $i$ . The following Table gives for assumed angles of incidence, decreasing by  $5^\circ$  from  $90^\circ$ , the number of plates required to make these angles the angles of maximum polarization of the transmitted light, and the value of  $\chi_1$ , which determines the defect of polarization.

$i =$	$90^\circ$	$85^\circ$	$80^\circ$	$75^\circ$	$70^\circ$	$65^\circ$	$60^\circ$	$56^\circ 40' (= \varpi)$
$m =$	1	1.330	1.944	2.913	4.921	9.775	30.372	$\infty$
$\chi_1 =$	.433	.422	.390	.337	.265	.177	.075	0

V. "On the Theory of the Polyhedra." By the Rev. T. P. KIRKMAN, M.A., F.R.S.

This is a revised version of a Paper having the same title, read on the 30th of May, 1861, of which an abstract has been already given at page 218.

*January 30, 1862.*

Major-General SABINE, R.A., President in the Chair.

The following communications were read:—

- I. "On the Calculus of Symbols."—Second Memoir. By W. H. L. RUSSELL, Esq., A.B. Communicated by ARTHUR CAYLEY, Esq. Received January 7, 1862.

(Abstract.)

This memoir is the continuation of one on the calculus of symbols

which I had the honour to lay before the Society in December 1860, and which has since been published in the 'Philosophical Transactions.' I commence this paper with some extensions of the method given in the former memoir for resolving functions of non-commutative symbols into binomial factors. I then explain a method, analogous to the process for extracting the square root in ordinary algebra, for resolving such functions into equal factors. I next investigate a process for finding the highest common internal divisor of two functions of non-commutative symbols, or, in other words, of finding if two linear differential equations admit of a common solution. After this, I give a rule for multiplying linear factors of non-commutative symbols, analogous to the ordinary algebraical rule for linear algebraical factors. I then resume the consideration of the binomial theorem explained in the former memoir. Two new forms of this binomial theorem are here given; and the method by which these forms are proved identical will, I hope, be considered an interesting portion of symbolical algebra, and as exhibiting in a remarkable manner its peculiar nature.

II. "On Internal and External Division in the Calculus of Symbols." By WILLIAM SPOTTISWOODE, Esq., M.A., F.R.S. Received January 8, 1862.

(Abstract).

Continuing my researches in the calculus of symbols, I have been led to investigate the most general case of division, viz. that wherein a function of any degree  $n$  in  $\pi$  is divided, (1) internally, (2) externally, by another function of any other degree  $m$  in  $\pi$ . The investigations here subjoined give (1) the various terms of the quotient, together with their laws of derivation both by actual division and otherwise; (2) the final remainder, and thence the conditions that the divisor may be a factor, internal or external as the case may be, of the dividend. An example has been added in each case by way of illustrating the processes. A remarkable reciprocal relation subsisting between the functions ( $\Phi$ ), of the coefficients ( $\phi$ ) of the dividend, and the corresponding functions ( $\Psi$ ) of the coefficients ( $\psi$ ) of the divisor is exhibited, at the end of the paper.



I have confined myself throughout to that branch of the calculus wherein the functions treated of are arranged according to powers of  $\pi$ ; that wherein they are arranged according to powers of  $\rho$  has been already more fully discussed by Mr. Russell.

III. "On the Absorption and Radiation of Heat by Gaseous Matter."—Second Memoir. By JOHN TYNDALL, Esq., F.R.S. Received January 9, 1862.

(Abstract.)

Resuming with a new apparatus his experiments on the influence of chemical combination on the absorption and radiation of heat by gases, the author in the present investigation first examines the deportment of chlorine as compared with hydrochloric acid, and of bromine as compared with hydrobromic acid, and finds that the act of combination, which in each of these two cases notably diminishes the density of the gas, and renders the coloured gas perfectly transparent to light, renders it more opaque for obscure heat. He also draws attention to the fact that sulphur, which is partially opaque to light, is transparent to 54 per cent. of the rays issuing from a source of  $100^{\circ}$  C., while its compound, heavy spar, which is sensibly transparent to light, is quite opaque to the rays from a source of  $100^{\circ}$  C. He demonstrates, in confirmation of Melloni, the transparency of lampblack in thin layers, and shows how irreconcilable its deportment to radiant heat is with the idea generally prevalent at the present day, that lampblack absorbs heat of all kinds with the same intensity.

He has repeated all his experiments with gases, using a different source of heat, and finds the result still more pronounced than formerly, that the compound gases far transcend the elementary ones in absorptive power. Taking air as unity, ammonia at 30 inches tension is 1195,—this latter figure representing *all the heat* that issued from the source. A layer of ammonia 3 feet long is *perfectly black* to heat emanating from an obscure source. The coloured gases chlorine and bromine, though much superior in absorptive power to the transparent elementary gases, are exceeded in this respect by every compound gas that has been hitherto examined. When instead

of tensions of 30 inches we compare tensions of 1 inch, the differences between the gases come out still more strikingly. At this tension, for example, the absorption of sulphurous acid is eight thousand times that of air.

The author also records a new and extensive series of experiments on the absorption of radiant heat by vapours. The least energetic, as before, he finds to be bisulphide of carbon, the most energetic boracic ether. He shows that the absorption of the latter vapour, which is quite transparent, is, at 0.1 of an inch of tension, 600 times the absorption of the densely coloured vapour of bromine, while in all probability it is 186,000 times that of air.

The author was led by a series of perplexing experiments, which are fully described in the memoir, to the solution of the following remarkable and at first sight utterly paradoxical problem, "*To determine the absorption and radiation of a gas or vapour without any source of heat external to the gaseous body itself.*"

When air enters a vacuum, it is heated by the arrest of its motion: when a vessel containing air is exhausted by an air-pump, chilling is produced by the application of a portion of the heat of the air to generate *vis viva*. Let us call the heating in the first case dynamic heating, and the chilling in the second case dynamic chilling. Let us further call the radiation of a gas which has been heated dynamically, dynamic radiation, and the absorption by a gas which has been chilled dynamically, dynamic absorption. A thermo-electric pile being placed at the end of the experimental tube, and the latter being exhausted, the gas to be examined is permitted to enter: the gas is heated, and if it possess any sensible radiative power, the pile will receive its radiation and the galvanometer connected with the pile will declare it.

Proceeding in this way with gases, the author found that the radiation thus manifested, and which was sometimes so intense as to urge the needle of the galvanometer through an arc of more than sixty degrees, followed the exact order of the absorptions which he had already determined. After the heat of the radiating column of gas had wasted itself by radiation, the air-pump was worked at a certain rate; the rarefied gas within the tube became chilled, and the face of the pile turned towards the chilled gas became correspondingly lowered in temperature. The dynamic absorptions of

various gases were thus determined, and they were found to go strictly hand in hand with the dynamic radiation.

In the case of vapours the following method was pursued. A quantity of the vapour sufficient to depress the mercury column 0.5 of an inch was admitted into the tube, and this was heated dynamically by allowing dry air to enter until the tube was filled. The radiation of the vapours thus determined followed exactly the same order as the absorption which had already been measured. The dynamic absorption of the vapour was obtained by pumping out in the manner just described, and it was found to follow the same order as the dynamic radiation. In these experiments the air bore the same relationship to the vapour that a polished silver surface does to a coat of varnish laid over it. Neither the silver nor the air, both of which are elements, or mixtures of elements, possesses the power of agitating in any marked degree the luminiferous ether. But the motion of the silver being communicated to the varnish and the motion of the air being communicated to the vapour, molecules are agitated which have the power of disturbing in a very considerable degree the ether in which they swing.

The author shows, by strict experiments, that the dynamic radiation of an amount of boracic ether vapour possessing a tension of only  $\frac{1}{101250000}$ th of an atmosphere is easily measurable. He also shows, and explains the fact, that with a tube 33 inches long the dynamic radiation of acetic ether considerably exceeds that of olefiant gas, while in a tube 3 inches long the dynamic radiation of olefiant gas considerably exceeds that of the ether. Aqueous vapour has been subjected to a special examination, and the author finds it a common fact for the aqueous vapour contained in the atmosphere to exercise 60 times the absorption of the air itself. In fact, the further he has pursued his attempts to obtain perfectly pure and dry air, the more has the air approached the character of a vacuum. The author further points to the possibility of determining the temperature of space by direct experiment.

Scents of various kinds have been examined. Dry air was passed over bibulous paper moistened by the essential oils and carried into the experimental tube. Small as the amount of matter here entering the tube is known to be, it was found that the absorption by those odours of radiant heat varies from 30 times to 372 times that of the air

which formed its vehicle. In fact the author remarks that the absorption of terrestrial rays by the odour of a flower-bed may exceed in amount that of the entire oxygen and nitrogen of the atmosphere above the bed:

Ozone has also been subjected to examination. The substance was obtained by the electrolysis of water, and from decomposing cells containing electrodes of various sizes. Calling the action of the ordinary oxygen which entered the experimental tube with the ozone unity, the absorption of the ozone itself was in six different experiments 21; 36; 47; 65; 85; 136. The augmenting action of the ozone accompanied *the diminution of the size of the electrodes* used in the decomposing cells. The author points out the perfect correspondence of these results with those of M. Meidinger by a totally different method of experiment. The paper contains various reflections on the nature of this remarkable substance.

February 6, 1862.

Major-General SABINE, R.A., President, in the Chair.

The following communications were read :—

- I. "Remarks upon the most correct Methods of Inquiry in reference to Pulsation, Respiration, Urinary Products, Weight of the Body, and Food." By EDWARD SMITH, M.D., LL.B., F.R.S., Assistant Physician to the Hospital for Consumption, &c., Brompton. Received January 9, 1862.

Having been engaged in several researches into the vital actions of the human system, which have extended over lengthened periods, I have necessarily formed opinions as to the best methods of inquiry, and have noticed some circumstances which tend to induce incorrect results. On consideration it has appeared to me that it might serve the interests of science as much to solicit the attention of present and future investigators to the circumstances connected with the mode of inquiry, as to adduce the facts which the inquiries have elicited; for it cannot be doubted that nearly all the errors which have found place in this department of physiology have been due to deficiency in the methods of inquiry, whereby only a part of the results arrived at were

obtained from actual observation ; and hence much valuable labour has been lost and science has been led into erroneous channels. I have therefore ventured to lay before the Royal Society, with a view to publication in its ' Proceedings,' a short summary of the conditions which I believe to be essential to the elimination of truthful results in inquiries connected with the rate of pulsation and respiration, the quantity of air inspired, of carbonic acid expired, and of urinary water and urea excreted, the weight of the body, and the influence of foods ; and in so doing I purpose first to consider the inquiries into the daily quantities of each of them, and then to refer to each subject separately.

### 1. *The determination of the daily quantities.*

In none of the subjects for inquiry will observations made at one or a few periods of the day enable us to infer the total daily quantities, since there are scarcely any two hours in which the quantities remain unchanged ; and although the variations, for the most part, follow a definite course, the progression is not so uniform that, even under the most favourable circumstances, they may be safely inferred.

The rate of pulsation and respiration varies in such a manner that it is increased after each meal during about two hours, and then declines for about an equal period, unless food be in the meantime taken. There is a less proportionate increase and decrease after the early dinner and tea than after breakfast, and at about 8 or 9 P.M. the rate falls rapidly and continually until the middle hours of the night. The nearest approach to a stationary rate occurs—1st, in the middle hours of the night, with a tendency to increase ; 2nd, after rising and before breakfast, with a tendency to decrease ; and 3rd, in the afternoon, when the midday meal has been deferred, with a tendency to increase at the usual meal-hour. The least stationary periods are the three or four hours following each meal, and the late hours in the evening. In the total absence of food throughout a whole day, the rate remains nearly stationary from the hour of rising in the morning until about 9 P.M., with a tendency to change at the usual meal-hours.

The quantity of air inspired and of carbonic acid expired is subject to similar variations ; but the increase after the early dinner is less, and that after tea is greater than that of the rate of pulsation, and the rate is more constant before breakfast and throughout a day of fasting.

The rate of evolution of urea is less uniform than that of either of

the above-mentioned subjects of inquiry, since it depends more upon the conditions of the preceding day, and upon the variations in the amount of fluid ingesta. It is, however, the least during the night, and then in the morning before breakfast, but it is not stationary at the latter period. It rapidly increases after breakfast for the space of about three hours, and then decreases even more rapidly, and continues low, notwithstanding the early dinner, until the tea hour, after which it again rises, and finally it falls at about 9 or 10 P.M. until the night-rate is attained.

The rate of production of urinary water follows the order of that of the emission of urea, except that the increase in the afternoon is much less, and there is an approach to uniformity of production after about 3 P.M.

Hence, with the exception of the excretion of urea after the early dinner and of urinary water in the afternoon, there is a progressive increase followed by a progressive decrease in all the subjects of inquiry after each meal, and therefore several such alternations occur during each day. There is also a low rate in the night and immediately before each meal.

The only correct method of determining the daily quantities is to collect the whole. This may be effected at rest, or with mixed rest and exertion, in reference to the urea and urinary water. So also in reference to the air inspired and carbonic acid expired, except that as to the latter the degree of exertion must be limited, and attention to the daily duties of life nearly intermitted. I do not think that the total number of pulsations and respirations can be recorded except by the process of counting, since the registering instruments are liable to fail, and an error cannot be corrected. The nearest approach to correct results, short of the foregoing, will be made by observations taken at regular and frequent periods during the 24 hours; but the period of intermission must not exceed one hour, and it need not be more than a quarter of an hour.

A rough estimate may be obtained by taking the average of the four maxima and four minima of the rate of pulsation and respiration, and the quantity of air and carbonic acid, if the inquiry be limited to the 18 working hours of the day, viz. 6 A.M. to midnight. If the meals be taken at  $8\frac{1}{2}$  A.M.,  $1\frac{1}{2}$ ,  $5\frac{1}{2}$ , and  $8\frac{1}{2}$  P.M., the periods of inquiry will be 8 and  $10\frac{1}{2}$  A.M., 1,  $3\frac{1}{2}$ , 5, 7, 8, and 10 P.M.

The determination of percentage quantities is worthless for this purpose, unless the total quantities of air or urinary water be also ascertained and employed in the calculation. As the per-centage quantities vary greatly, it is necessary that the daily quantities be determined to a uniform hour; and the best period is that which immediately precedes breakfast.

## 2. *General considerations affecting each subject of inquiry.*

### PULSATION AND RESPIRATION\*.

In instituting any inquiries into the rate of pulsation and respiration, the following conditions are required :—

The posture of the body must be uniform; and as the sitting posture has a rate nearly intermediate between that of lying and standing, it should be universally preferred, except in inquiries into the influence of sleep, or on the sick often confined to the couch, when inquiries in the lying posture are alone practicable. The posture should remain quite unchanged during the whole time of the inquiry, also for at least five minutes before its commencement, if the person has been previously at rest, and fifteen minutes in the case of previous exertion. The attention must be withdrawn. Care should be given to ascertain if the rate is influenced by a feeling of nervousness, or by any other disturbing cause; and if so, the inquiry must be deferred until the rate has become uniform. There are also conditions, however absurd it may appear, when the observer is liable to mistake the pulsation in his thumb or finger for that of the person under inquiry. The rate should be counted during two minutes if practicable, and in no instance less than one minute. Half a respiration should be recorded; and the counting should be commenced only from a long line on the watch dial. It is often very difficult to count the respirations during quiet sleep in the night, and even during wakefulness, in many women, notwithstanding close observation of the movements of the *alæ* of the nose and of the upper part of the chest; and in such cases the hand must be slightly applied to the chest. Coughing, yawning, and dreaming temporarily accelerate the pulse. It occasionally occurs that the rate of respiration or pulsation becomes doubled or halved; and intermitting pulsation often occurs in children and feeble persons during sleep, whether by night or day. In comparing the rates upon

\* See *Medico-Chirurgical Transactions*, vol. xxxix. 1856.

different days, care must be taken that the inquiries are all made at rest, at the same hour, in the same posture, and at the same period after meals ; and even then the results will be liable to great variation. No comparison can be made of the rates at different periods of the year, nor indeed on any two consecutive days, if there have been any considerable atmospheric changes or material variation of the daily habits. With increase of temperature the rate of pulsation increases, whilst that of respiration declines. The effect of moderate exertion continues for several minutes, and of severe exertion for about half an hour. An unusual rapidity of pulsation in one half of the day will be attended by the opposite state in the other half, if the conditions have been changed at those periods,—as, for example, an increase from exertion, followed by rest, or a decrease from fasting, followed by food. Extreme differences of 30 pulsations per minute occur in the 24 hours. The chief objection to the use of instruments to register the rate is their liability to miss the record from the difficulty of closely applying them to the wrist or chest, and the great variations in the extent of pulsatory and respiratory movement in sleep and wakefulness.

The usual practice amongst medical men of ascertaining the rate of pulsation, regardless of posture of the body, of meals, and of the hour of the day, is evidently liable to the greatest error. In order to obtain even approximately correct results, the visit should be made at about the same hour daily, and the inquiry pursued always in one posture.

#### QUANTITY OF AIR INSPIRED \*.

The same attention to posture is required as that noted under the previous heading. It is more satisfactory to measure the inspired than the expired air, since no correction is required for expansion by heat. Moreover, when the object is to ascertain the vital capacity of the lungs, it is important to remember that the inspiratory force at the end of a deep inspiration is greater than the expiratory force at the end of a deep expiration. It is necessary to cover or close the nose, since otherwise a portion of air is unconsciously inhaled through it. If a tube be inserted into the mouth for the purpose of inspiration, the lips must be closely pressed against it, and particularly at the angles of the mouth ; but there are many persons who cannot

\* See Phil. Trans. 1859.



close the lips perfectly with a tube placed in the mouth. When it is desired to measure ordinary inspiration, it is advisable to use a mask which encloses the chin, nose, and mouth, so that inspiration may proceed easily and naturally through both openings. The sides of the mask must be made of lead, so thick that when pressed upon the features it will retain its position; and it should be lined with sheet caoutchouc, the better to adhere to the skin. The face must be well introduced into the mask, and the thumbs placed under the chin whilst the forefingers cover the free edge of the mask so as to press the lead to the face and prevent any ingress or egress of the air either at the bridge of the nose or on the sides. Persons with large beards, and those with very thin and sunken cheeks, cannot use the mask effectually. After the mask has been worn for some time the vapour condenses within it, and the fluid trickles beneath the chin, when it will be very difficult to prevent a little air entering until the mask shall have been removed and wiped. The mask may be held upon the features by bands which cross the head transversely and longitudinally. Care must be taken that the valves close well and act easily. If the mask has been laid aside for some time, the valves will have curled up; and it will be necessary to place it in lukewarm water for half an hour, or to pour water upon the dry valves. The measuring-instrument should offer but a very small amount of resistance to the expired current of air; so that, if it be a gas-holder, as in Hutchinson's and Davy's spirometer, it should be accurately counterpoised at every part of its progress; or if it measure and register ordinary inspiration, the adverse pressure should not exceed  $\frac{2}{10}$ ths of an inch of a column of water. The person must breathe normally, by the aid of previous training and by abstracting the attention. If there should be any sense of constriction about the chest, it may be inferred that the respiration is not normal, and that the chest is either too much collapsed, so that the act of expiration is too prolonged, or it remains expanded above the ordinary degree, and the act of expiration is shortened. The sense of ease and satisfactory respiration must be at all times present. Hence practice and intelligence are necessary. The experiments must embrace several minutes at a time; for a respiration rarely ends at a complete minute, and must therefore be recorded as a fraction, and the attention cannot be abstracted in the short period of one minute. Results obtained from

observations of so short a duration cannot be uniform. The aim must be to ascertain the precise number of respirations and the quantity of air inspired then occurring. To control the respiration by inspiring a predetermined number of times per minute, ~~on~~ by breathing with an assumed uniformity of depth, is to render the act and the results alike unnatural. It is doubtless possible to fix the number of respirations, but it is impossible to regulate the quantity of air inspired except by the aid of a spirometer. The effect of exertion may be accurately determined by fixing the mask upon the face with bands, and by carrying the spirometer in the arms, or fastened upon the back with knapsack straps. The instrument must not exceed a very few pounds in weight. The distance to be traversed must be accurately measured, and subdivided into short distances also accurately measured, so that, with the watch in hand, the rate may be tested every half minute and over every small part of the course. Each subdivision should be a known part of a mile, and at the rate selected must be traversed in a given number of seconds. Thus, at the rate of two miles per hour, a course of  $58\frac{1}{2}$  yards would be traversed every minute; and if that be subdivided into six equal parts, each one would be walked over in a very little less than ten seconds.

#### THE CARBONIC ACID EXPIRED\*.

The remarks already made in reference to posture and rest are also applicable to this subject. It is impossible so to regulate the respiration that a fair average of the carbonic acid evolved may be made from inquiries of one or two minutes' duration, both from the impossibility of withdrawing the attention, and of obtaining an exact proportion of the expiration in so short a time. We almost always found that the rate of respiration was greater during the first than during subsequent minutes; and this was no doubt attended by a change in the quantity of carbonic acid expired, and it was due to the action of the mind. Five minutes is the shortest period during which such an inquiry should be continuous, and ten minutes would be better if the duration of each experiment should not interfere with the necessary frequency of repetition; but the latter might be an essential character of the inquiry. An inquiry of five minutes duration may be repeated every twelve minutes. The apparatus required must be

\* See Phil. Trans. 1859.

capable of containing the products of respiration during five or ten minutes, or must absorb them as fast as they are emitted. There must be no adverse pressure upon the respiration. In collecting the expired air in a bag, there will be the fallacy of not being able to empty the bag completely; and unless special care be taken, there will be an adverse pressure from the weight of the sides of the bag, and from their cohesion. Moreover, it is impossible to measure the expired air by such means; and if it be passed through a spirometer, there will be a fallacy from the pressure required to move the instrument; or if it be passed into a graduated tube, there will be a change of bulk from temperature and pressure. If only a part of the collected air be submitted to analysis, it will be very difficult to obtain a fair sample, since the specific gravities of the component gases vary much. In seeking the absorption of the gas, it is essential that the expired air should not be forced through a layer of fluid, since the adverse pressure upon the respiration would cause either defective expiration or an increased effort to expire, and in both cases error, but in opposite directions, would occur. No arrangement of solid absorbents with moistened surface can be so made that it shall absorb all the carbonic acid during the process of expiration. No combination of tubes within tubes, with a view to increase the absorbing surface, can be arranged within a manageable space and weight suited to this purpose. Hence it is requisite that the expired air be passed *over* a fluid; and the fluid must be capable of rapidly and certainly absorbing the gas, and offer so large a surface that it may be found by experiment capable of absorbing the whole of the carbonic acid during the period of expiration. The air should be exposed in thin layers to the surface of the absorbent, and only a small column of it be offered at the same moment, so as to allow a long period to elapse before each small portion of the expired air shall have traversed the whole surface. A test apparatus should be attached at the end, and the test be occasionally applied during the inquiry, so as to ascertain if any unknown cause of error exists. Any portion of the absorbing fluid which may have been carried along by the current of the previously dried air must be arrested before the air escapes into the atmosphere, and no element of the expired air, besides the carbonic acid which the absorbent might retain, must be allowed to enter the absorbing apparatus. By this method the amount of carbonic acid

is determined by the increase of the weight of the absorbing apparatus; and hence it is necessary that the latter be such that it may be weighed to not less than the  $\frac{1}{10}$ th of a grain. Gutta percha is the only known substance of which the apparatus may be constructed, since it is not greatly acted upon by the caustic alkali, may be readily formed by the aid of the hot iron, and does not allow the fluid to flow over the floors of the chamber with the readiness observed when glass or metal is used, during the act of weighing. The only practical absorbent is a solution of caustic potash, and its specific gravity should be 1.270. The atmospheric air which will enter the various parts of the apparatus when not in use, must be expelled by blowing expired air through them before they are counterpoised as a preliminary to the inquiry. The expired air must not be re-inspired. Hence boxes such as that employed by Scharling are inapplicable; for, however rapid the current of air which is drawn through the box, it is quite certain that some portion of the air will be again and again inspired, and moreover the dilution of the air charged with carbonic acid renders the absorption of the gas much more difficult, whilst the determination of the carbonic acid remaining in the box is always a circumstance of great difficulty. When a mask is worn, it should not have a capacity larger than necessary to contain the features, or it will retain expired air, which must be re-inspired. When respiring through a tube placed in the mouth, it is exceedingly difficult to prevent the escape of air, whether as it is introduced into or withdrawn from the mouth, and the results cannot be relied upon. If the nose be left unclosed, a variable and unknown quantity of air will enter and leave by that aperture. The effect of exertion may be readily ascertained by using a tube 15 feet in length attached to the mask and the analytical apparatus. The apparatus must be placed in a central position, and a space of 30 feet marked out in a right line, and this must be walked over at a defined rate of speed. The potash-box must be made of larger size than that required for experiments upon quiet respiration, or two sets of the apparatus must be used at the same time. In the latter case there will be danger of adverse pressure from the air passing through so many vessels; and if the exertion in breathing be considerable, it will be impossible to measure the inspired air at the same time. The tubes must be of sufficient diameter and of smooth material; and be filled with expired air before the experi-

ment commences. These observations also apply to experiments upon voluntary respiration, where the force, depth, and rapidity exceed that at rest. A space of 1000 to 1500 superficial inches of absorbing surface will be necessary with exertion, whilst 700 inches is sufficient at rest.

There is a variation in the quantity of carbonic acid expired on different days of the week, so that there is an increase after a day of rest. There is also a variation with the season of the year, so that the evolution is the greatest in the spring, then in the winter, and then in the autumn, and it is the least at the end of summer. It increases with cold, and decreases with heat. Hence the quantities evolved at various seasons cannot be compared, neither indeed those in a short period of days, if there have been any considerable changes of weather or habits. The only mode by which the rate of evolution in different days and in different seasons may be compared is by making observations in the morning before food has been taken, and in absolute rest, so as to isolate the effects of season and meteorological phenomena from every other influence except the small effect of the conditions of the previous day. This method is almost without fallacy.

#### URINARY WATER AND UREA\*.

The urinary water should be collected in tall and narrow glasses which are graduated to  $\frac{1}{10}$ th of an ounce, and covered to prevent evaporation. When travelling,  $\frac{1}{10}$ th of the quantity emitted at a time may be reserved, and the larger portion thrown away. The glasses must be used during defæcation; and hence such inquiry cannot be accurately made in women. The various quantities must be retained and collected to the exact termination of each 24 hours; and after they have been mixed and reduced to the temperature of the air, a sample should be taken for analysis. As the urine decomposes readily in warm weather and when the specific gravity is very low, the analysis must not be deferred later than two days; but in the opposite conditions four days may elapse.

There are very great and rapid variations in the quantity of urine evolved; so that large and small quantities may alternate daily, or a sequence of increase or decrease may be established, or one may follow the other for many days. Hence a correct daily average can

\* See Phil. Trans. 1861.

only be obtained after perhaps ten days of inquiry : and the quantity observed in one season will not apply to that of any other season ; so that the effect of season can only be ascertained by continued inquiries through the year. Rest, diminished ingestion of fluid, increased ingestion of animal solids, increased temperature and atmospheric pressure, profuse discharges from the skin or bowels, and increased bulk of the body from whatever cause, all other things being equal, will lessen the excretion of urine. The contrary conditions, each one, other things being equal, will increase the excretion of urine. The action of any two of these agents, each in an opposite direction, will modify the influence of the other. It must not be inferred that there will be lessened excretion of urine because there is increased excretion of fluid by other outlets, as the skin, unless it be proved that there was no increase in the quantity of fluid ingested and no diminution in the bulk and weight of body. Urinary water is largely and quickly excreted when fluid is drank without any solids having been taken on that day, viz. before breakfast, but to a much less extent if solids have been previously taken, and still less when solids are taken with the fluid. The maximum rate of emission must be sought for between the breakfast and 1 P.M., and by experiments made not less frequently than a quarter of an hour. The minimum quantities occur in the night, and continue for much longer periods. All graduated glasses, alkalimeters, and pipettes should be graduated and carefully proved by the observer before using them, and this may be conveniently and most accurately effected by the balance.

In pursuing Liebig's volumetric method for the determination of urea in the urine, it is essential that the operator have graduated the mercurial solution himself, or have made himself familiarly acquainted with the tint of colour to which it is graduated by repeatedly testing it with the proper quantities of pure urea. Moreover, as the recollection of the precise tint to which the solution was graduated fades from the memory, the test quantity of urea should be used from time to time to renew it. The urea to be used must be proved to be perfectly pure. The solution should be made in a quantity of several gallons, and be drawn from the carboy by a siphon with the smallest apertures, so that the standard strength may be preserved. The quantity in daily use for the supply of the alkalimeter should not

exceed a few ounces, and the alkalimeter must be washed out with distilled water after each operation has been finished. Care must be taken to force out the bubble of air which is retained in the neck of the exit-pipe before the operation begins. If it be possible, the analyses should always be made in the same amount and kind of light, since otherwise there will be an incorrect perception of the proper tint. The direct rays of the sun, and even too bright an indirect light, must be avoided as much as a deficiency of light. It is not possible to use artificial light. The thickness of the layer of the solution of carbonate of soda should be uniform, since there will be a difference in the tint and the rapidity of its production in the shallower and the deeper parts. From one to two minutes must be allowed for the production of the colour when it approaches the standard tint. Dr. Guy's spatula is to be preferred to Dr. Beale's suction-tube, since it retains a less quantity of the thick fluid from the previous immersion. It often occurs, in the analysis of coloured urine (as in urine of high specific gravity), that the distinction of the tint is not well appreciated if the solution be added in quantities of one division only; and hence it is often better for the experienced investigator to add two divisions of the solution at a time, so as to produce a little excess of colour, and then to compute and deduct the excess. In urine of low specific gravity, the tint is quickly and distinctly produced by half a division of the solution. The specific gravity of healthy urine is a ready guide to the addition of the first and large quantity of the solution. In a healthy person, and one of regular habits and under ordinary conditions, the daily quantity of chloride of sodium which is eliminated and must be deducted from the urea is tolerably uniform; and the quantity having been ascertained by numerous trials, it may be used for the same person as a constant quantity, where absolute accuracy is not essential, and thus the labour will be materially lessened. With urine of a specific gravity from 1012 to 1025, it is convenient to add by the pipette  $\frac{1}{2}$  an ounce of urine to  $\frac{1}{2}$  ounce of the baryta solution. When the specific gravity exceeds 1025 (in the absence of sugar), equal parts should be used; when it is below 1012, it is needful to add 3 or 4 parts of urine to 1 part of baryta solution, and with diabetic urine 4 parts should always be added. A quarter of an ounce of the mixed fluids should be taken with the pipette, and the number of divisions of the mercurial solution used to produce the

tint must be multiplied by the following factors to determine the amount of urea per ounce :—

With equal parts of each multiply by.....	8
With 2 parts of urine and 1 part of solution of baryta } multiply by .....	6
With 3 parts of urine and 1 part of solution of baryta } multiply by .....	5.33
With 4 parts of urine and 1 part of solution of baryta } multiply by .....	5

Considerable and constant practice is essential to correct and comparable results.

The periods of the formation and the elimination of urea are different, and there is no known method of showing the former. The urea from metamorphosis of tissue and from the transformation of food is a mixed and varying product, and the two sources cannot be dissociated. The direct relation of urea is with food, since, in the absence of exertion, it nearly represents the nitrogen in the food supplied, less that remaining in the fæces. The elimination of urea chiefly varies with the quantity of urine, and therefore will be influenced by the same agencies as affect the discharge of urine. Hence the duration of inquiries to determine the normal daily rate of elimination at distant periods of the year, must be the same as that indicated in reference to the urine. There are great and frequent variations in the daily elimination of urea in a person of the most regular habits; and as the effect of any agent is often carried on to the following day, inquiries which may be made for a short period before breakfast will not faithfully represent the conditions of that day.

#### WEIGHT OF BODY\*.

The only satisfactory method of determining the weight of a person day by day, is to weigh him naked directly after he has passed urine and before he has taken any ingesta, and to do so as nearly as possible at the same hour every morning. The error which will be due to the varying amount of fæces contained in the bowel will still exist, but it cannot be large, and by no method can it be entirely removed. The person cannot weigh himself unless stand-scales with a multi-

\* See Phil. Trans. 1861.



plying lever be employed. It will usually suffice to weigh to  $\frac{1}{2}$  or 1 oz. The weight of the body will be influenced by the quantity of food taken, and by all the circumstances already noted in reference to the emission of urine. Hence the body is heavier at night than in the morning, also after a day of rest than after labour, in warm than in cold weather, and in all conditions in which the bulk of the body is increased, and the elimination of fluid lessened. There is a close relation between sudden changes in the quantity of urine evolved and the weight of the body. Among the excretions which cause a variation in the weight of the body, the carbonic acid, although a gas, must not be overlooked; and so far the weight will vary as the conditions above mentioned vary the production of carbonic acid. Hence, upon the whole, the determination of the weight at night is attended by greater liability to error than in the morning, and the latter period would alone suffice for the inquiry. A variation in the weight occurs almost daily, and under some circumstances it amounts to from 1 to 2 lbs. The varying weight of the body represents the varying quantity of the fluid and solid excretions, the fat and the fluids in the blood-vessels and tissues, besides the nitrogenous elements of the body. Under the discipline of a prison, there is the highest proportion of nitrogenous tissues to the weight of the body.

#### FOOD\*.

The effect of food upon the system may be sought in two ways:—  
1st, the general effect of the ordinary dietary, which will represent the actual condition of the body in the individual or in the masses, but not the separate influence of any food. This is of great importance when considered in relation to the community, and may show its actual state under ordinary conditions. For this purpose the methods of inquiry already referred to under the different subjects will suffice.  
2nd, the effect of separate articles of food only. This can only be ascertained in the absence of every agent acting upon the system, except the one in question. Hence the food must be taken alone, and before any other food has been eaten on that day, viz. before breakfast. The inquiry must also be made with precisely a uniform degree of exertion, and therefore at rest only, and in the absence of all excitement and meteorological changes. If an unusual kind or

\* See Phil. Trans. 1859.

quantity of food be given, it will probably disturb the system and give inaccurate results. It is necessary to give a moderate dose, and in the customary form. The effect of all agents is temporary, and that of all kinds of food begins quickly and attains its maximum within 1 to 2½ hours. If the maximum effect only be sought for, that period will suffice for the inquiry; but if the average or total influence be desired, it will be necessary to continue the inquiry until the whole period of increase and decrease, or *vice versa*, have passed over. In either case the experiments must be made every few minutes, and be regularly repeated. The maximum quantities are easily attainable, but the true average or the total effect is scarcely if at all so, since it is difficult or impossible to ascertain the precise period of the termination of the effect. Hence only one dose of the food can be given on the same day, when great accuracy is desired. A second period may be found at about 4½ hours after the breakfast; but, although it is next in value to the period before breakfast, it cannot be implicitly relied upon, since no proof could be obtained that the vital functions had subsided from the breakfast increase to their lowest point before the inquiry began. All such experiments must be tested by morning inquiries. Whenever there is a sense of craving for food, or any disturbed feeling, it is highly probable that the vital actions are varying, apart from the influence of the food, and the inquiry should be terminated. The addition of water to the food does not vary the results connected with the respiration, except so far as it may enable the food to enter the circulation quickly. If the solution of the food have been imperfect, the subsequent ingestion of water alone will cause an increase in the effect equal to that of taking more food.

## II. "On the Motions of Camphor on the Surface of Water."

By CHARLES TOMLINSON, Esq., Lecturer on Science,  
King's College School, London. Communicated by Dr.  
WILLIAM ALLEN MILLER, Treasurer and V.P.R.S. Re-  
ceived January 15, 1862.

(Abstract.)

The object of this paper is to show that the phenomenon in question is a much more general one than is commonly supposed; that

the explanations hitherto given of it have been insufficient or erroneous. The author endeavours to explain the real nature of the phenomenon in a series of experiments and observations, and to establish the following propositions :—

I. That the camphors, or stearoptens of the volatile oils, present phenomena of rotation and progression when thrown on the surface of clean water in a chemically clean vessel.

II. That these phenomena belong also to certain salts, and to a variety of vegetable and other substances containing a liquid that diffuses readily over the surface of water.

III. That solutions of camphor in benzole, in some of the essential oils, &c., present phenomena of rotation and progression on the surface of water—a property which also belongs to creosote, and to some other liquids that do not contain camphor.

IV. That the motions of camphor may be imitated by placing on water miniature rafts or coracles of inert substances, such as talc, tinfoil, paper, &c., smeared with or containing the elæoptens of volatile oils, or indeed any volatile liquid, such as ether, alcohol, chloroform, &c., provided there be some communication and adhesion between such liquid and the surface of the water.

V. That the camphors, &c., being slightly soluble in water, that is, the adhesion of the water partly overcoming the cohesion of the camphor, a film of camphor is thus detached from it, and spread over the surface of the water the moment that the camphor comes in contact therewith.

VI. That the dimensions and form of this film depend on those of the piece of camphor operated on ; and, in general, the film separates more easily from broken surfaces and angles of the fragment than from a smooth natural surface, just as the crushed or broken surface of a crystal is more soluble than a perfect crystal.

VII. That such films being constantly detached from the camphor so long as it is in contact with the water, displace each other ; the preceding film being conveyed away by the adhesion of the water in radial lines, these produce motion, by reaction on the fragment, causing it to rotate after the manner of a Barker's mill.

VIII. That these radial lines or jets being of unequal intensity, the direction and intensity of the motion will follow that of their resultant.

IX. That the jets or films of camphor can be rendered sensible by various means—as by fixing the camphor partly submerged in water, and dusting the surface lightly with lycopodium powder: a series of horizontal currents produced by the films will then be made visible, which films or jets cause the camphor, when free to move, to rotate on a vertical axis.

X. That the motions of the fragments of camphor on water are greatly influenced and complicated by their mutual attraction and by the attraction of the sides of the vessel.

XI. That the film of camphor diffused over the surface of the water is very volatile, disappearing as fast as it is formed, chiefly into the air, only a very small portion being retained by the water. Hence camphor wastes away much more quickly at the surface of the water than in water alone or in air alone, because at the surface the film is being constantly formed at the expense of the camphor, and is spread out to the united action of air and water.

XII. That whatever interferes with evaporation lowers or arrests the motions of the camphor and the allied phenomena; so, on the contrary, whatever promotes evaporation exalts these phenomena. Effects which are displayed with great energy on a bright and sunny day, are produced either sluggishly or not at all on a wet, dull, or foggy one.

XIII. That a fixed oil forming a film on water will displace the camphor film, and so permanently arrest the motions of the camphor; but a volatile oil will only arrest the motions while it is present and undergoing evaporation.

XIV. That the presence of the camphor film on water will, in some cases, prevent the formation of other films, the liquids that would otherwise form them remaining lenticular.

XV. That the camphor film, and other films, in many cases repel each other on the surface of water.

XVI. That the motions of camphor on the surface of water are accelerated by the action of the vapour of benzole, and some other volatile substances: such vapours, condensing in the liquid form on the camphor, and then being diffused by the adhesion of the water, react on the camphor.

*February 13, 1862.*

Major-General SABINE, President, in the Chair.

The following communication was read :—

“On Magnetic Calms and Earth-Currents.” By CHARLES V. WALKER, Esq., F.R.S., F.R.A.S. &c. Received February 8, 1862.

(Abstract.)

The author uses the word “calm” in a negative sense, “not storm”, and states that very few notable earth-currents have attracted attention since the date of his original communication to the Royal Society, which was read on February 14, 1861.

Referring to that communication, he calls attention to the London, Tonbridge, and Dover-London lines of telegraph, making an angle of direction with each other of  $149^{\circ}$ , and by means of which a few groups of observations were made, from which the prevailing direction of earth-currents was determined to be approximately N.E. or S.W. He wished to multiply these observations and to modify them, which he was well able to do from the circumstance that the Dover-London telegraph wires enter his own private office at Tonbridge, where, by means of the necessary apparatus, he is able at any moment, when the wires are not occupied by telegrams, to obtain possession of the whole wire from end to end, Dover-London; or either section, London-Tonbridge, or Dover-Tonbridge,—the two former being the limiting lines, or those making the greatest available angle with each other, and the last, which is intermediate, being useful in confirmation of the observations made on the other two.

The telegraph needles have been rarely affected of late, the earth-currents which form the subject of the present communication being feeble. In order to their examination it was therefore necessary to prepare a delicate galvanometer, which is properly connected with the telegraph wire, and furnished with the simplest possible apparatus for bringing it into action whenever occasion serves. It is within arm's length of the author when in his office. The pressing-down of a spring allows any earth-current that may be present to enter the galvanometer; a brass plug, placed in holes 1, 2, and 3, gives

possession of the whole line, or either of its sections. The needle is deflected on the side marked "up" or "down," according as the current collected is moving up or down the line; and in all the Tables given in this communication the letters "*u*" or "*d*," placed beside the degrees of deflection, give the direction of the current in the above popular terms. The galvanometer, with its appliances, accompanies this communication, and is placed on the table as *in situ*, and will at a glance give an idea of the arrangement, of which also the author gives a plan.

A Table is given of earth-currents collected at Tonbridge, in October 1861, on the lines in question, together with the Meteorological Register of the month. An analysis of these observations follows, included with which is an analysis of all observations of a like character that were made in the subsequent month of November.

A few cases are recorded in which the earth gave no sign of current. No stress is laid on this, because a closer investigation with more delicate instruments might have given positive results.

The contents of the Table are divided into *Normal*, *Abnormal*, and *Exceptional*. Out of a total of 276 observations, 230 gave *normal* results, confirming the conclusion already arrived at, that the prevailing direction of earth-currents was approximately N.E. or S.W. Whether one or other of these directions prevailed more or less at different periods of the day did not appear, the observations not being sufficiently consecutive. Father Secchi's views of the relation between meteorological phenomena and magnetic variation are referred to. The author has reason to conceive that sunshine or cloud, heat or cold, influence the relative values of the current collected from different parts of the same district; in connexion with which he refers to a group of night observations, which form part of the series made in October, and also to the want of consistency in the relation between two derived currents collected at the same time from different parts of the same plane. He gives a few extracts from the Table, showing how very variable are the relations; for instance,  $15^{\circ} : 15^{\circ}$ ;  $15^{\circ} : 30^{\circ}$ ;  $15^{\circ} : 35^{\circ}$ ;  $13^{\circ} : 38^{\circ}$ ;  $18^{\circ} : 21^{\circ}$ ; and so on.

Professor Loomis's "Eighth Article" on the subject is referred to; and the correspondence between the results at which he arrives by other processes, and those to which Mr. Walker arrives by the methods herein described, are given.

In addition to the currents whose direction has been already noted, 42 cases occurred of currents which, for distinction sake, are called *Abnormal*, and which were equally definite in character. They are found in the S.E. and N.W. quadrants; but the probable place in these quadrants could not be determined with any approach to accuracy from the lack of other lines of telegraph immediately at command. Four diagrams are given in illustration of the *normals* and *abnormals*.

The author mentions that the South-Eastern Railway Company have cordially entertained the proposition, to which he has previously referred, of the Astronomer Royal; and that he is now preparing to erect wires for Mr. Airy, terminating respectively near Dartford and Croydon, and which by combination will give an angle of  $36^\circ$  or  $107^\circ$ , the former, however, being without the range of normal direction. The consecutive observations to be made on these wires promise to be very instructive. The porcelain-ebonite insulator that will be used is described; a specimen is on the table.

Among the 276 October–November observations, four cases occurred which are exceptional and do not admit of similar discussion. Subsequent observations may explain these.

Next follows a survey of the N. and S. boundaries of a plane, the mean dimensions of which are 56 miles  $\times$  20 miles, bounded on the N. by the Thames, and on the S. by the Dover–Tonbridge line of railway. This was accomplished by aid of the earth-plates at Ramsgate and London, to the former of which access was had at Tonbridge, when required, by means of a switch at Ashford Junction. A Table is given of observations made during November and December, which show that the plane of the current is at least 20 miles wide, and the direction is consistent at either limit of the plane.

Tonbridge being very nearly midway on a line joining London and Hastings, gave the opportunity of making observations on the whole or on either half of a same line of country. The results collected in November and December are given in a Table, and show a conformity in direction in the whole and in both halves, but a marked excess in value in the London–Tonbridge as compared with the Hastings–Tonbridge section. These differences are considered by the author as probably due to the different geological conditions of the country on either side of Tonbridge. Sections kindly furnished

by Mr. Robert Hunt are referred to. These differences indicate the influence of local conditions, as the differences previously mentioned point at the interference of meteorological variations.

In order to satisfy himself that he was dealing with currents collected *bonâ fide* from the earth, and in no way from the atmosphere, arrangements were made with the clerks at Ashford to detach the observing wire from the earth there when required. A considerable number of observations were made during October, November, and December, the results of which are tabulated. Whether the current, as shown by the galvanometer, was weak or strong, it in every instance entirely ceased when the wire at Ashford was detached from the earth and held insulated; so that no portion of the result was derived from any other source than the earth. These observations were made at all periods of the day and night, and in all weathers.

Powerful artificial currents were repeatedly made to flow into the earth by the earth-plates, in order to see whether any effects of polarization were produced; but the value of the earth-current, as observed before any such experiment, remained unchanged.

That the currents collected are in no way due to the electromotive power of the earth-plates themselves, is shown by the absence of any sign of a tendency for one or other direction. They are independent in character and in value of all such influences. To prevent misconception, a list of the earth-connexions used at the several stations that enter into the present investigation is given.

The author considers it may be premature to regard the subject as tolerably exhausted, as far as the means at his command are concerned; but at this moment he does not notice any other salient point within his reach. When the proposed special wires are ready for Mr. Airy, and consecutive observations are made and compared with the march of the magnetometers, the subject will be within the reach of the able hands of Mr. Airy; and we may be well assured that the various questions connected with it will be ably discussed by him.

The results comprehended in this and the previous communication are briefly summed up as follows:—

- 1st. That currents of electricity are at all times moving in definite directions in the earth.
- 2nd. That their direction is not determined by local causes.



3rd. That there is no apparent difference, except in degree, between the currents collected in times of great magnetic disturbance and those collected during the ordinary calm periods.

4th. That the prevailing directions of earth-currents, or the currents of most frequent occurrence, are approximately N.E. and S.W. respectively.

5th. That there is no marked difference in frequency, duration, or value, between the N.E. and the S.W. currents.

6th. That (at least during calm periods) there are definite currents of less frequency from some place in the S.E. and N.W. quadrants respectively.

7th. That the direction of a current in one part of a plane on the earth's surface (at least as far as the S.E. district of England is concerned) coincides with the direction in another part of the plane; and if the direction changes in one part, it changes in all parts of the plane.

8th. That the relation in value between currents in a given part of the plane and currents in another given part is not constant, but is influenced by local meteorological conditions, and varies from time to time.

9th. That the value of the current of a given length, moving in a given line of direction, is not necessarily the same as of a current of the same length on the same line of direction produced, and that their relative value depends on the physical character of the earth interposed between the respective points of observation, and is tolerably constant.

10th. That the currents which have formed the basis of these investigations are derived currents from true and proper earth-currents, and neither in whole nor in any appreciable part have been collected from the atmosphere, nor are due either in whole or in any appreciable part to polarization imparted to earth-plates by the previous passage of earth-currents or of powerful telegraphic currents; nor are they due to any electromotive force in the earth-plates themselves.

11th. That the earth-currents in question (at least the powerful currents present at all times of great magnetic disturbance) exercise a *direct* action upon magnetometers, just as artificial currents confined to a wire exercise a direct action upon a magnet.

February 20, 1862.

Major-General SABINE, President, in the Chair.

The following communication was read:—

“On the Dicynodont Reptilia, with a Description of some Fossil Remains brought by H.R.H. Prince Alfred from South Africa in November 1860.” By Professor R. OWEN, F.R.S. &c. Received January 23, 1862.

(Abstract.)

In this paper the author describes some fossil remains obtained, at the suggestion of H.R.H. the late Prince Consort, by H.R.H. Prince Alfred, during his journey in South Africa. They are referable to two genera of Dicynodont Reptilia. The first specimen is an unusually perfect specimen of the skull, retaining the lower jaw in connexion with the tympanic pedicles, of a species of *Ptychognathus*, showing distinctive characters from previously described species, and which the author dedicates to its discoverer under the name of *Ptychognathus Alfredi*. The anatomical characters of this fossil were described in detail. It was obtained from a greenish sandstone, probably Triassic, of the Rhenosterberg, South Africa.

The second specimen is the skull, with the lower jaw, also *in situ*, of a true *Dicynodon*, referable by its size to the largest known species (*Dicynodon tigriceps*, Ow.). The right maxillary and zygomatic arch having been partially removed in quarrying the rock containing the fossil, a further detachment of the matrix brought into view the descending cranial plate of the frontal, the inter-orbital septum, the upper surface of part of the bony palate with the pterygoid, and the rhinencephalic continuation of the cranial cavity. The presphenoid projects forward as a compressed plate, exceeding in relative length and extent of ossification that in *Chelonina*, and more resembling that in *Crocodylia*. Anterior to the presphenoid is the vomer, which expands laterally to join the palatines and pterygoids. Other cranial characters deducible from the present and not shown in previous specimens are noticed. As a whole, the skull exemplifies the near equality in size of this extinct two-tusked reptile of South Africa with the existing Walrus; and it

shows that in the structure of the bony palate, as in some other parts of the skull, the *Dicynodon* combines Crocodilian with Chelonian and Lacertian characters.

The specimen above described was obtained by H.R.H. Prince Alfred, from the Karoo beds, in the district of Graaf Reinet, South Africa.

The author next proceeds to describe the pelvis of a *Dicynodon* equalling in bulk the *D. tigriceps*, and most probably belonging to that species. It includes, with five sacral vertebræ, the last of those of the trunk which supported free ribs, showing that there are no vertebræ having the character of lumbar ones in *Dicynodon*. The length of the six successive centrums was 1 foot 2 inches. The ribs of the first sacral vertebra resemble in size and shape the human scapula, but are much thicker; their expanded terminations, 6 inches in breadth, underlap or pass anterior to the iliac bones, to which this rib has been attached by syndesmosis. The ribs of the succeeding sacral vertebræ are shorter and thicker, and abut against the ossa innominata, as far back as the ischial tuberosities. The ilium, ischium, and pubis have coalesced to form one bone, as in some lizards and in mammalia; and, as in the latter class, the symphysis at which the ischio-pubic portion of each os innominatum joins its fellow is continuous; the pubic symphysis is not separated from the ischial symphysis. But ossification has advanced further than in any mammal, to the complete obliteration of the obturator foramina, which in most reptiles are represented by very wide vacuities. The pubic bones show an oblique perforation near the acetabulum, homologous with that which co-exists with large obturator openings in most lizards. The brim of this singularly massive pelvis measures 10 inches in antero-posterior, and 11 inches in transverse diameter: the outlet measures 4 inches in antero-posterior, and 9 inches in transverse diameter.

In the comparison of this, at present, unique type of pelvic structure, it is interesting to observe, in connexion with the mammalian tusks in the skull, a mammalian condition of the symphysis pubis, and also a mammalian expansion of the iliac bone. In the number of sacral vertebræ *Dicynodon* resembles the Dinosaurian reptiles, as well as some mammalia; and hence it may be inferred that, like the *Megalosaurus* and *Iguanodon*, a heavy trunk was in part supported

on a pair of large hind limbs, the weight thereupon being transferred by a larger proportion of the vertebral column than in the prone crawling crocodiles and lizards of the present day.

The author, from certain associated fossils, deduces a probability of the triassic age of the sandstones including the above-described South African Reptilia, and remarks that it is in a sandstone of triassic age in Shropshire where fossil remains occur of a reptile which, in biting with trenchant edentulous jaws, also pierced its prey by a pair of produced weapons analogous to the tusks of *Dicynodon*. Of this reptile, the *Rhynchosaurus articeps*, Ow., the author describes the skull, vertebræ, and some other bones, which have been lately discovered in the New Red Sandstone of Grinsill, Shrewsbury. The remains of the limb-bones in this specimen bespeak a reptile capable of progression on dry land, as well as of swimming in the sea—of one that might leave impressions of its foot-prints on a tidal shore.

This paper is illustrated by numerous drawings.

*February 27, 1862.*

Major-General SABINE, President, in the Chair.

The following communications were read:—

- I. "Notices of some Conclusions derived from the Photographic Records of the Kew Declinometer, in the years 1858, 1859, 1860, and 1861." By Major-General EDWARD SABINE, P.R.S. Received February 6, 1862.

The discussion of the magnetic observations which have been made in different parts of the globe may now be considered to have established the three following important conclusions in regard to the magnetic disturbances: viz., 1. That these phenomena, whether of the declination, inclination, or total force, are subject in their mean effects to periodical laws, which determine their relative frequency and amount at different hours of the day and night. 2. That the disturbances which occasion westerly and those which occasion easterly deflections of the compass-needle, those which increase and those which decrease the inclination, and those which increase and those which decrease the magnetic force have all distinct and generally different periodical laws. 3. That there exists a periodical

variation in the relative amount of disturbance in different years, constituting a cycle of about ten terrestrial years, which has been found to correspond, both in the duration of the period and in the epochs of maxima and minima, with a periodical variation in the appearance of spots on the solar disk.

In the introductions prefixed to the several volumes containing the observations made at the colonial observatories, the concurrent testimony of the disturbances of the three magnetic elements to these conclusions is fully exhibited; and in reference particularly to the third, viz. the decennial variation, a *résumé* has been given in the second St. Helena volume, pages cxxii to cxxxvi.

In that *résumé*, the particular form of the previously announced decennial variation is more fully traced, and, from the analysis of the observations, shown to be of the following character. If we begin with the part of the cycle to which the maximum of disturbance belongs, we find, first, three consecutive years in each of which the aggregate amount of disturbance (measured from a constant value) is nearly the same; then, two years of diminished disturbance; and then, three years in each of which the aggregate amount is nearly the same, but is considerably less than in the two preceding years, and very considerably less than in the three commencing years. The three years of minimum are then succeeded by two of medium disturbance, and these by the recommencement of three years of maximum amount. Thus, for example, referring to the years in which the colonial observatories were in action, 1841 and 1842 were years of medium disturbance; 1843, 1844, and 1845 years of minimum, differing little from each other; 1846 and 1847 years of medium, and 1848, 1849, and 1850 years of maximum. The general analogy of these particular features with Schwabe's observations of the solar spots, commenced in 1826 (showing, on the one hand, the number of groups of spots, and on the other hand, the number of days free from spots in each year), may be examined by a reference to the table in the third volume of 'Cosmos' (English translation), page 292, and is as satisfactory as, from the nature of the subject, could well be expected\*.

\* The variation in the aggregate amount of disturbance in the years of minimum, 1843, 1844, and 1845, compared with the two years of medium immediately preceding, 1841 and 1842, and with the two years of medium immediately

The discontinuance of the colonial observatories occasioned a temporary suspension of investigations which are now admitted to have been of very high interest; but by the liberality and public spirit of the British Association, and by the aid of occasional grants of money from the Royal Society, apparatus for their resumption was completed at the Kew Observatory in 1857, and the investigations were recommenced on the 1st of January, 1858. The results obtained from the photographic records of the Kew declinometer in 1858 and 1859, with a full description of the methods and processes employed in their elicitation, were communicated to the Royal Society in 1860, and are printed in vol. x. of the 'Proceedings,' pp. 624-643. The two years which have since elapsed have furnished similar results for the years 1860 and 1861, strictly comparable with those of 1858 and 1859, having been obtained with the same instruments and by the same methods. We have now, therefore, the observations of four consecutive years from the Kew Observatory, and we are thereby enabled to infer, by the comparison of the aggregate amount of disturbance in each of those years, the progression of the decennial variation up to the close of 1861. The aggregate amounts of disturbance in the four years were severally as follows:—

1858, January 1 to December 31,	7263·7 mins. of arc.
1859,                               "                               "	7637·3                               "
1860,                               "                               "	7540·2                               "
1861,                               "                               "	6461·6                               "

The observations of preceding years had led to the expectation that 1858, 1859, and 1860 would be the three years of maximum, in which succeeding, 1846 and 1847, is well shown by the results of the hourly observations made in those years at the Hobarton Magnetic Observatory.

				Minutes of arc.		
1841, Jan. 1 to Dec. 31; aggregate values	5441·9			} 4761·4.		
1842, " " "	4080·8					
1843, " " "	2183·4			} 2565·2.		
1844, " " "	2948·6					
1845, " " "	2563·7					
1846, " " "	3735·5			} 4309·5.		
1847, " " "	4883·4					

The aggregate values which are here given are the amounts in each year of the disturbances exceeding 2'13, reckoned from the normals of the several months and hours.

the aggregate amounts of disturbance would differ but little, and that 1861 would be the first year of medium, showing an aggregate amount of disturbance considerably below 1858, 1859, and 1860. This expectation has been realized; and we have now before us the prospect that the present year, 1862, will prove to be the second year of medium, with an aggregate amount of disturbance nearly resembling that in 1861, but a little less; and that 1863, 1864, and 1865 will be years of minimum, differing little from each other in the amount of disturbance, and all lower than the preceding years 1861 and 1862 on the one hand, or the succeeding years 1866 and 1867 on the other. Hence we see the importance of maintaining, during the remaining portion of the decennial period, the photographic records of the Kew Observatory, with as little change as may be practicable in the instruments and methods which have been employed during the first portion.

The Table which is printed in vol. x. of the Proceedings, page 627, shows the aggregate values of the disturbances in 1858 and 1859 distributed into the several solar hours of their occurrence, and distinguishing between the disturbances which produce westerly and those which produce easterly deflections of the compass-needle. It also exhibits the ratios of disturbance at the several hours to the mean of the 24 hours taken as the unit. The subjoined Table contains the same particulars for the *four* years, 1858 to 1861, inclusive. It has of course a somewhat higher authority than the earlier table, inasmuch as ratios obtained from the records of four years are to be preferred to those derived from two years only. But the principal point of interest in comparing them with each other is the evidence which their correspondence affords, of the substantial truth of the two first of the three general conclusions adverted to in the commencement of the present communication, viz., the periodicity of the disturbances in respect to the several hours of solar time, and the distinct character of the laws which regulate the disturbances producing westerly deflections, and those producing easterly deflections. The principal features of both classes of disturbance are the same, whether viewed in the record of the two or of the four years. Regarded from either point of view, both classes follow progressions manifestly dependent upon the hours of solar time, the progressions of the westerly and those of the easterly deflections being as manifestly governed by distinct and

different laws. The westerly deflections have their chief prevalence from 5 A.M. to 5 P.M., or during the hours of the day, the ratios at all the other hours being below unity. The easterly deflections, on the other hand, prevail chiefly during the hours of the night, the ratios being for the most part below unity at the hours when the westerly are above unity, and, conversely, above when the westerly are below. The easterly have one decided maximum at 11 P.M., towards which they steadily and continuously progress from 5 P.M., and from which they as steadily, and continuously, recede until 5 A.M. the following morning. The westerly appear in both records to have a double maximum, one about 6 or 7 A.M., the other about 2 or 3 P.M.

TABLE showing the aggregate Values of the larger Disturbances of the Declination at the different hours of solar time in 1858, 1859, 1860 and 1861, derived from the Kew Photographic Records; with the Ratios of Disturbance at the several hours to the mean hourly value taken as the Unit.

Local astronomical time.	Westerly deflections in four years.		Easterly deflections in four years.		Ratios in two years.		Local civil time.
	Aggregate values.	Ratios.	Aggregate values.	Ratios.	Westerly.	Easterly.	
Hours.	Min. of arc.		Min. of arc.				Hours.
18	860.5	1.55	210.9	0.33	1.85	0.37	6 A.M.
19	904.9	1.63	221.1	0.34	1.83	0.38	7 A.M.
20	769.7	1.38	219.8	0.34	1.48	0.36	8 A.M.
21	732.1	1.32	234.3	0.36	1.23	0.38	9 A.M.
22	640.0	1.15	245.7	0.38	1.26	0.33	10 A.M.
23	696.3	1.25	228.9	0.35	1.21	0.39	11 A.M.
0	855.5	1.54	234.7	0.36	1.38	0.54	Noon.
1	947.0	1.70	218.3	0.34	1.44	0.48	1 P.M.
2	941.6	1.69	263.6	0.41	1.53	0.54	2 P.M.
3	954.5	1.72	197.5	0.30	1.71	0.34	3 P.M.
4	847.1	1.52	265.7	0.41	1.35	0.44	4 P.M.
5	595.1	1.07	332.6	0.51	1.15	0.51	5 P.M.
6	458.7	0.82	477.8	0.74	0.94	0.91	6 P.M.
7	272.0	0.49	798.6	1.23	0.37	1.19	7 P.M.
8	196.0	0.35	962.6	1.49	0.22	1.56	8 P.M.
9	230.9	0.42	1184.1	1.83	0.29	1.79	9 P.M.
10	148.6	0.27	1512.8	2.33	0.20	2.25	10 P.M.
11	121.9	0.22	1615.2	2.49	0.12	2.38	11 P.M.
12	266.5	0.48	1471.3	2.27	0.49	2.21	Midnight.
13	245.7	0.44	1352.7	2.09	0.47	1.98	1 A.M.
14	306.7	0.55	1291.9	1.99	0.49	1.80	2 A.M.
15	287.6	0.52	988.0	1.52	0.51	1.45	3 A.M.
16	407.1	0.73	702.7	1.08	0.97	0.95	4 A.M.
17	662.9	1.19	322.5	0.50	1.53	0.45	5 A.M.
Mean hourly values		556.2 = 1.00	648.1 = 100				



The main object of the Table is to exhibit the amounts of disturbance and the ratios at the several hours, derived from the photographic records of the four years; but, in order to facilitate the examination of the correspondence in these respects of the results severally deducible from the two and from the four years, the ratios of westerly and of easterly disturbance at the different hours which were derived from the photographic records in 1858 and 1859 are added, being reproduced from the table in vol. x.

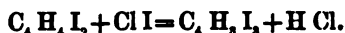
In discussions published elsewhere the preponderance of westerly over easterly deflection, or the converse, has been inferred to be a *geographical characteristic* rather than an accidental feature. All the stations in North America, at which investigations have hitherto been made, concur in showing a considerable predominance of easterly deflections, whilst at Peking in Northern Asia the converse is observable. Regarding Kew as the only representative station in the British Islands (the only one in which this investigation has been made), it is deserving of notice, that we find in this locality no constant or decided predominance of either class of disturbance over the other. There is indeed a slight preponderance of easterly values on the average of the four years, but not of such amount or regularity as to give it the character of a decided feature.

## II. "On the Action of Chloride of Iodine on Iodide of Ethylene and Propylene Gas." By MAXWELL SIMPSON, M.B. Communicated by Dr. FRANKLAND. Received February 18, 1862.

I have already shown\* that the cyanides of the diatomic radicals can be prepared by submitting their bromides to the action of cyanide of potassium. In the hope of forming the cyanides of the triatomic radicals in a similar manner, I subjected the bromides of several of these latter radicals to the action of the same reagent. Finding, however, the reaction not quite satisfactory, it occurred to me that the iodides of these radicals might possibly yield better results. With this view I endeavoured to prepare the teriodide of aldehydene ( $C_4 H_2 I_3$ ), by exposing iodide of ethylene to the action of chloride

\* Philosophical Transactions, 1861, p. 61.

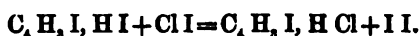
of iodine, expecting that the teriodide would be formed by virtue of the following reaction :—



My expectations, however, were not realized, the product of the reaction being a body which I shall call chloriodide of ethylene ( $\text{C}_4\text{H}_4\text{I Cl}$ ). The experiment was performed in the following manner :—

*Action of Chloride of Iodine on Iodide of Ethylene.*—A solution of chloride of iodine in water containing a trace of free iodine was agitated vigorously with a quantity of iodide of ethylene, till the latter became black and changed into a fluid oil. This was then washed with dilute potash and distilled. Almost the entire liquid passed over between  $146^\circ$  and  $152^\circ$  Cent. It gave, on analysis, results which correspond perfectly with the formula I have given above. I obtained 12.43 instead of 12.55 per cent. of carbon.

If we regard the constitution of iodide of ethylene as  $\text{C}_4\text{H}_4\text{I}_2$ ,  $\text{HI}$ , the reaction which gives birth to this body becomes perfectly intelligible. It is simply the substitution of chlorine for iodine in hydriodic acid :—



Chloriodide of ethylidene is a colourless oil. It has a sweet taste, and is slightly soluble in water. It boils at about  $147^\circ$  Cent. It is a remarkable fact that neither this body nor Dutch liquid is formed when iodide of ethylene is exposed to the action of chlorine-water.

I have also subjected propylene gas, derived from amylic alcohol, to the action of chloride of iodine, and find that an oily body is formed in large quantity, which contains iodine. This I am at present engaged in studying. The action of chloride of iodine on propylene gas obtained from glycerine appears to be similar.

Letter to the Council from Sir George Everest, C.B., On the Expediency of re-examining the Southern Portion of the Great Indian Arc of the Meridian ; and Report of a Committee thereupon.

[Towards the close of the last session of the Royal Society a letter was addressed to the President and Council by Colonel Sir George Everest, C.B., advocating the expediency of re-examining the portion of the Great Indian Arc of Meridian which was surveyed by the

late Colonel Lambton, and collecting in one volume the results of that part of the survey. The Council, having taken this letter into consideration, appointed a committee, consisting of the Astronomer-Royal, Professor W. H. Miller, and Professor Stokes, to consider and report on the subject. The report was laid before the President and Council early in the present session ; and by their direction the letter of Sir George Everest and the report of the Committee are here printed.]

*Letter of Colonel Sir George Everest, C.B.*

10 Westbourne Street, Hyde Park, W.  
April 8th, 1861.

SIR,—In a letter which I took occasion to address to you some time back \*, some remarks are made to which I am desirous to draw the attention of the President and gentlemen of the Council of the Royal Society. They are contained in page 7 of the printed copy of that letter ; and as they relate to a subject of considerable importance in the estimation of myself and many others, I hope no apology will be necessary for the present intrusion.

To enter into a long narrative of my reasons for the statements therein made would but be to repeat what I have frequently urged on other occasions ; but in this place it will perhaps be sufficient to mention that, 1st, the details of such portions of the late Colonel Lambton's operations on the Great Arc of India to the south of Damargida, as have been printed, are only to be found in a dispersed state in the volumes of the Asiatic Researches of Calcutta ; and if it is intended that these should be permanent data, they ought to be collated and combined into one volume, in keeping with that relating to the portion north of Damargida, which was printed by me in 1847, at the expense and by the desire of the late East India Company.

2nd. The details of all trigonometrical operations conducted by Colonel Lambton are to be found in manuscript, in the copies of what are denominated the General Reports of the Great Trigonometrical Survey of India, which are deposited amongst the records at the India House ; and as, in transcribing, there is always a liability to clerical errors, therefore a volume such as is here suggested ought to be drawn up after a rigorous comparison with the manuscript ; and further, wherever it may be practicable, the observations

\* Proceedings of the R. S., Jan. 27, 1859.

registered in the General Reports should be compared with those originally noted in the Field- or, as they are called, Angle-Books of the department.

3rd. There certainly has been one error, if not more than one, committed in the computations ; but where such error or errors exist it is impossible to say *à priori* : the only decisive mode of detection must consist in a thorough recomputation.

4th. All the celestial observations for amplitude made by Colonel Lambton were reduced many years ago ; but I need hardly point out that the constants and formulæ for aberration, precession, nutation, &c., have undergone vast alterations since that period, and of course corresponding recomputations would now be necessary. This would not have been needed if the observations at each limit of the Arc of Amplitude had been made simultaneously by two instruments on the same set of stars, one instrument at each limit, as has been done in the two Arcs north of Damargida ; but it becomes of importance when not only the years, but the seasons of observations were different.

If this were effected, we should at least have the satisfaction of knowing that the most had been made of the late Colonel Lambton's operations, which indeed might fairly rank with those of MM. Bouguer and De la Condamine, or MM. Maupertius, Clairaut, and others, though, from the inferiority of instruments and other causes, of course they could not be classed for accuracy with those of a more modern date.

In proper time and place I have abundance to say on this subject ; but it will be evident that the revision and recomputation here suggested constitute a task beyond the power of any individual, and are indeed a state affair, which, now that India and all belonging to it has been taken under the control of Her Majesty's Government, can only be accomplished as other state concerns usually are.

What, therefore, I venture to recommend is, that the President and gentlemen of the Council of the Royal Society should take this subject into their consideration, as a national question falling peculiarly under their superintendence, and that in their capacity as the parent Society and leading scientific body of Great Britain, they should use their influence to have such measures effected as in their judgment may seem meet.

Perhaps a recommendation from the Royal Society to the Secretary

of State for India would be the proper course to be pursued ; but in any case it seems very clear that it is not creditable to leave this subject in its present disjointed state. India furnishes the largest extent of territory accessible to Great Britain in which arcs of the meridian can be measured, and there can be no question that from Cape Comorin to the Himalayan Mountains one uniform triangulation ought to be formed.

The most effectual method of accomplishing this desirable purpose would assuredly have been that which I counselled the Government of India to adopt in 1842 ; but as my proposal was rejected, it only remains to make the most of the materials we actually possess.

As to now giving effect to my proposal, which was to revise the whole series south of Damargida with the same instruments and observers as had been employed in the northern portion, there would be difficulties which did not then exist. Not to speak of the fact that there are none of the observers of that day at present available, it must be remembered that the station-marks of the Bedu base and the Damargida Observatory were then fresh and intact, as were indeed the other station-marks in general ; but the natives of India have a habit peculiar to human beings in that state of society, of attributing supernatural and miraculous powers to our instruments, and the sites which have been occupied by them. In cases of death or any other natural visitations they often offer up prayers to those sites ; and if the object of their prayers be not conceded, they proceed to all sorts of acts of destruction and indignity towards them : nay, as in all cases where it was practicable, my station-marks were engraved on the solid rock *in situ*, they have been known to proceed in bodies armed with sledge-hammers, and beat out every vestige of the engraving ; so that it is by no means certain that the marks which designate the limits at Damargida and Bedu could now be detected.

I will not trouble you with any further remarks, but, with full confidence that the Royal Society will, after giving the subject due consideration, take such measures as the case may seem in their wisdom to require,

I beg to subscribe myself, Sir,

Your very obedient Servant,

GEORGE EVEREST.

*To the Secretary of the Royal Society.*

*Report of the Committee.*

The Committee to whom it was referred by Minute of the President and Council of the Royal Society of the date of June 13, 1861, to consider and report on a letter by Colonel Sir George Everest, C.B., dated April 8, 1861, relating to the steps proper now to be taken in reference to Colonel Lambton's Survey of an Arc of Meridian in India, and on the subjects therewith connected, have to offer the following Report:—

1. The Committee have examined the principal printed books on the subject, namely,

The several volumes of the Asiatic Transactions, containing the details, to the extent to which in works of similar character they are usually published, of Colonel Lambton's Surveys.

The recalculation of the celestial amplitudes by Bessel in No. 334 of the 'Astronomische Nachrichten.'

The two printed volumes by Sir George Everest, containing the details of his own Indian Survey with much information on Colonel Lambton's Survey.

A former letter addressed by Sir George Everest to the Secretary, and printed in the 'Proceedings of the Royal Society' for January 27, 1859 (vol. ix. pp. 620–625).

The Committee have also been favoured by Sir George Everest, at a personal interview which that gentleman at their request most kindly granted them, with very important oral information on the instruments, the methods of proceeding, and other particulars relating to Colonel Lambton's and to his own survey; and they have been permitted by him to peruse a most valuable document, partly of private and partly of semi-official character, addressed to him by Mr. De Penning, formerly Chief Assistant to Colonel Lambton in the conduct of the Survey.

2. The Committee will first advert to the observations and primary deductions from them (of the nature of adopted angles, &c.) in Colonel Lambton's surveys. And in regard to these, they have no hesitation in stating their opinion that no good whatever would be done by general examination of the angle-books. It is evident from Mr. De Penning's statements that the utmost care was used, and the best judgment of the Officers was exercised, at a time when all the

qualifying circumstances of the separate observations were known to them, and that any attempt to depart from their conclusions at the present time would probably lead to error.

The Committee remark that the exhibition (in the Asiatic Researches) of the adopted angles with the corrections required to make the sum of the angles in each triangle equal to two right angles, renders it impossible that any clerical or typographical error can escape discovery: if any such should be found, of which the proper correction is not obvious and certain, they think it proper that reference should be made to the manuscripts now preserved in the Archives of the Department of State for India; but they recommend nothing further.

3. In regard to the accuracy of the calculations of the sides of the triangles, founded on the adopted angles to which allusion is made above, there appears to be no check except the verifications by the measure of widely-separated bases; and the comparison of these, as presented in the Asiatic Researches, shows a degree of accordance which the Committee, guided by the results of Sir George Everest's experience, consider satisfactory. Still they remark that the form in which these calculations are printed makes their verification extremely easy, and the Committee recommend that they be verified. Of the next step of calculation, namely the computation and aggregation of successive portions of the meridian (including the astronomical determinations of azimuth), there appears to be no check whatever; and the Committee recommend that this important calculation be repeated, and in a different form, if the officer entrusted with such revision should think it desirable.

4. The details of the base-measure reductions, as founded on Colonel Lambton's statements of the measuring process, admit of easy verification; and the Committee recommend that they be verified. But the evaluation of all these measures, for application to the estimate of the length of Arc of Meridian at the level of the sea, requires that the elevation of the bases be very approximately known. The portions of the Arc surveyed respectively by Colonel Lambton and Sir George Everest, join each other at Damargida; and there is a large discordance between the elevation of this station, as given first by Colonel Lambton, and secondly by Sir George Everest and Sir A. Waugh. Guided by the information which Sir George Everest

has furnished, on the inadequacy of the vertical circles of the instruments employed by Colonel Lambton, on the want of attention to atmospheric circumstances, and on the want of simultaneity in reciprocal observations (all which considerations have been carefully kept in view in Sir George Everest's and Sir A. Waugh's observations), the Committee recommend that Colonel Lambton's determinations of height of base be rejected, and those of Sir George Everest and Sir A. Waugh be adopted; and that the resulting corrections be made to the estimated lengths of Meridian Arcs, as far as, in the judgment of the Officer revising this work, it is now possible to do it.

5. The reductions of astronomical observations for celestial amplitude of arcs and absolute determination of latitude admit of easy examination; and the Committee recommend that they be thoroughly verified. The Committee recommend that the original numbers of these observations, as well as those of celestial azimuths, be verified by collation with any manuscripts of the Survey which may now be preserved in England.

6. The reduction of the Latitude-observations was corrected several years ago by Bessel. The Committee are of opinion that additional accuracy can now be given to these corrections. First, the proper motions of the stars are now better known than they were in Bessel's time. Secondly, the value of the coefficient of Nutation used by Bessel is now universally abandoned by astronomers. The alteration made in the result by the use of corrected values of these elements would probably be small; but, remarking that they can be introduced with great facility, the Committee recommend that the corrections be made.

7. The Committee have had personal experience of the great inconvenience caused by the dispersion of Colonel Lambton's accounts of the survey-operations through numerous volumes of the Asiatic Researches; and viewing the limited circulation of that work in continental libraries, they are inclined to believe that very few men of science have it in their power to form a correct judgment as to the value of Colonel Lambton's great work. The Committee therefore recommend that, when the verifications and corrections which they have particularized shall have been made, the whole be published in one volume, in a form as nearly similar as circumstances



permit to those describing Sir George Everest's operations and results, and in sufficient number (say 500 copies) to allow of their being presented to all the known Libraries, Academies, and Observatories of importance, throughout the world.

8. The Committee unhesitatingly express their opinion that the expense attending all the recommendations which they have made would be small in comparison with the scientific value of the result. And even in the event of ulterior operations (to which they proceed to allude) being ultimately sanctioned, the adoption of the course which they have recommended would give valuable facilities.

9. The Committee think it right, however, to call the attention of the President and Council to the general quality of Colonel Lambton's Surveys, which, though executed with the greatest care and ability, were carried on under serious difficulties, and at a time when instrumental appliances were far less complete than at present. There is no doubt that at the present time the Surveys admit of being improved in every part. The Standards of length are better ascertained than formerly, and all uncertainty on the unit of measure can be removed. The base-measuring apparatus can be improved. The instruments for horizontal angles used by Colonel Lambton were inferior to those now in use; and one of them was most severely injured by an accidental blow, the result of which was more distinctly injurious because the circle was read by only two microscopes. Allusion has already been made to the circumstances of observation affecting the altitude of stations. Though the astronomical observations were probably good for their age, yet new observations conducted with such instruments and on such principles as those adopted by Sir George Everest would undoubtedly be better. The Committee therefore express their strong hope that the whole of Colonel Lambton's Survey may be repeated with the best modern appliances. The expense of such a work would be considerable; but no Arc of Meridian yet measured has such claims on the attention of the patrons of science as the Indian Arc, from its proximately equatorial position, and from its anomalies and the reference of them to the attraction of the Himalaya Mountains.

"G. B. AIRY.

"W. H. MILLER.

"G. G. STOKES."

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